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Shaped by the past, creating the future

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Dear Dr Thornalley,

Thank you for your continued consideration of our manuscript entitled 'Re-evaluating the link between the sulphur-rich Laacher See volcanic eruption and the Younger Dryas' for publication in *Climate of the Past,* and for your comments. We feel that this manuscript is an extremely important submission that provides a straightforward but comprehensive explanation for the Younger Dryas event that will generally be well-received (as it already has been by two reviewers, and by other readers who have contacted us independently of the online system) and provocative, and we welcome the opportunity to submit a revised manuscript. The topic is also controversial, and we are therefore glad that it will go out to further review, which will undoubtedly help us present as compelling of a case as possible.

Below this cover letter is a point-by-point response to the comments by the reviewers, as well as to your editorial comments. Many of the responses are similar to those provided earlier in our responses to the reviewer's comments on the online discussion. You will note from the marked-up version of the manuscript (also included below) that the changes made are extensive, and in fact substantial changes have been made that were not requested in the review process, including a new figure that helps shift the emphasis from magnitude to sulphur yield (as suggested by Prof. Pyle). In many cases we provide examples of the new text that was included in response to the comment, but in some cases the changes were too extensive or unevenly distributed throughout the manuscript to be clearly listed, although of course the nature of the changes was always described. Please do let us know if you require any further details on any of the changes made, or if you have any other questions regarding our resubmission.

Best regards,

Dr James Baldini (Corresponding author)

Responses to Editorial, Reviewer, and short comments

Responses to Editorial comments:

Comment #1: Firstly, a couple of minor points: (a) as pointed out, the existence of MWP-1B is questioned (probably no abrupt jump in sea-level) - Bard et al 2010 Science.

Response #1: This has now been addressed and it is highlighted in the figure caption that "the source, timing, and even occurrence of the meltwater pulse are still debated", and in the main text that "this requires further research, particularly because the source, duration, timing, and even existence of MWP-1B are still unclear".

Comment #2: (b) Although this may not arise on your revised manuscript, the role of the AMOC in the Little Ice Age (LIA) has been questioned, with the latest reconstructions suggesting that AMOC did not weaken (Rahmstorf et al 2015, Nat. Clim. Change), and the LIA may instead be caused by changes in horizontal subpolar gyre circulation and sea-ice feedbacks (Moreno-Chamarro et al 2017, Clim. Dynamics).

Response #2: We have included the following text: "*Rahmstorf et al. (2015) argue that no identifiable change in AMOC occurred during the Little Ice Age, suggesting that the drivers were related to sea ice and atmospheric rather than oceanic, although this requires further research to confirm.*" We thank the editor for bringing this paper to our attention.

Although we see the relevance, we have opted to not include the Moreno-Chamarro et al 2017 paper because it relies exclusively on modelling results, which in our opinion appear very ambiguous. This paper of course relates to the LIA, and so is not directly relevant to the YD.

Comment #3: The broader issue with this submission is that it is not providing new data or evidence, but instead is using an updated age model to propose a new hypothesis (albeit one that had been earlier considered and rejected, as discussed). I am not overly concerned about this, since as stated by one of the reviewers, and highlighted by yourself, this is a topic that has perhaps been neglected and it is useful to put this idea out there to be discussed.

Response #3: We now realise that in fact we may have actually undersold the hypothesis in the previous submission. To our knowledge, no detailed discussion of the eruption as a YD trigger exists prior to this submission, only very brief mentions. Up until now, the LSE had been mentioned only in very oblique ways in papers focussing on other subjects. For example, Bogaard et al 1990 state "Whether the eruption of LSV contributed to the stage of climate deterioration known as the Younger Dryas….is a matter of current investigation" but that is the only mention of the YD in the paper, and as far as we are aware there was no further investigation. Possibly this was curtailed by research suggesting that the eruption occurred too early: in 2002 Baales et al. (Quat. Res.) state: "Correlation of terrestrial archives with the Greenland ice-core records and improved calibration of the radiocarbon timescale permit a precise, accurate age determination of the Laacher See event some 200 yr before the onset of the Younger Dryas cold episode" reflecting the thinking at the time. Even more enlightening is Schmincke et al. 1999 (Quat. Int.): "The Younger Dryas cooling period clearly was not triggered by LSE as formerly thought because it started ca. 180 years after the eruption."

These quotes reflect the fact that the Laacher See hypothesis was not previously developed, and also the lack of understanding that climate change associated with the YD was time transgressive. Therefore, we are the first to suggest the Laacher See hypothesis as a triggering mechanism in any meaningful way (to our knowledge). Although we considered changing the title, we still feel that the word 'Re-evaluating' is the correct word to use, given that earlier papers had discarded the possibility of the eruption as a trigger. The importance of this contribution is highlighted by a recent publication investigating a cometary airburst as the Younger Dryas trigger. Wolbach et al. 2018 (*Journal of Geology*) (incorrectly) state that: "Furthermore, Moore et al. (2017) examined three samples of tephra from the Laacher See eruption in Germany, which occurred 200 y before the cosmic-impact event and potentially could have contaminated the YDB layer with volcanic Pt." we now know that the LSE was coincidental with the YD, and did not precede it by 200 years, and yet a high-profile 2018 publication fails to consider that the eruption was coincident with the eruption, and could have contributed directly to the Pt spike discussed. We now discuss this history in more depth in the revised submission (and discuss the new Wolbach paper), including the following text:

"*Earlier research briefly alluded to the eruption as a possible causative mechanism for the YD (e.g., Berger, 1990; Bogaard et al., 1989). However, because the meltwater pulse hypothesis was already popularised, and because the effects of volcanic eruptions on climate would escape detailed quantification until after the 1991 Pinatubo eruption, the ~12.9 ka BP Laacher See eruption as a YD trigger never gained traction. Importantly, the concept was effectively dismissed after lacustrine evidence across central Europe appeared to indicate that the YD's clearest expression appeared ~200 years after the Laacher See Tephra within the same sediments (e.g., Brauer et al., 2008; Brauer et al., 1999a; Hajdas et al., 1995). For example, Schmincke et al. (1999) state that "The Younger Dryas cooling period clearly was not triggered by LSE as formerly thought because it started ca. 180 years after the eruption.", reflecting the accepted sequence of events at that time."*

Comment #4: However, it does mean that the merits of this manuscript depend heavily on it providing an accurate overview of the state of the art regarding the onset of the Younger Dryas (YD). In its current form I do not think the paper does this and there are major omissions in terms of providing the necessary context.

Response #4: We believe that the substantial revisions to the manuscript now present an extremely comprehensive overview of the onset of the YD.

Comment #5: The onset of the YD is associated with the end of the preceding Bolling-Allerod (B-A), which is usually considered to be a DO warm event/interstadial. There is a large body of literature that proposes the BA-YD oscillation is simply another DO cycle that occurred as climate passed through an intermediate climate state. Therefore the mechanisms involved for the BA to YD transition may be similar to those acting at the end of earlier DO interstadials. This is consistent with your assertion about the possible role of volcanic eruptions in ending both the BA and earlier DO interstadials. However, the duration of an interstadial has been shown to be dependent on its rate of cooling (ie DO interstadials do not end with random timing, and they occur after a period of gradual cooling). This evidence is difficult to reconcile with the suggestion that a volcanic event triggered the YD/stadial transition, in isolation. I therefore suggest that you consider a slight modification to your hypothesis, which stresses that climate was probably close to switching back to stadial conditions anyway, and that the occurrence of a volcanic eruption was the (small) trigger that determined the precise timing of the transition (which incidentally may help explain why only some stadials have been linked to volcanic eruptions in your earlier 2015 paper). In the context of the YD, many climate record, especially those around the North Atlantic and linked to the AMOC, show a gradual change (deterioration) in climate through the BA which fits with this generalized evolution of a DO interstadial.

Response #5: This is an excellent point, and one that we completely agree with. We have now included this in a discussion along with appropriate references. It is worth noting that the Toba eruption may also have occurred during a cooling trend, and we believe that in these situations a Northern Hemisphere eruption pushes the climate towards its insolation-mediated baseline (cold). This point is also critical for assessing an alternative hypothesis, that of a cometary airburst. Because only a minor trigger is required to shift the climate back to its pre-B-A glacial state, it is difficult to reconcile the fact that the Oldest Dryas was 4.5C colder than the YD with a catastrophic cometary airburst. Instead, it seems likely that the 'nudge' was reasonably minor. We include this new text in section 3.7 (Compatibility with other hypotheses) as a new discussion comparing the two hypotheses:

"*Another issue with the YDIH is that the YD was simply not that anomalous of a cold event, and therefore does not require an unusually powerful trigger. Over the last 120 ka, 26 Greenland Stadial (GS) events occurred (Rasmussen et al., 2014), of which the YD was the most recent. This does not exclude the possibility that the YD was forced by an impact event, but the most parsimonious explanation is that most stadial events had similar origins, implying a much more commonplace trigger than an impact. Furthermore, nitrogen isotopes suggest that Greenland temperature was 4.5ºC colder during the Oldest Dryas (18 to 14.7 ka BP) than the YD (Buizert et al., 2014), again suggesting the transition to the YD does not require an extreme forcing. It is also worth noting that the YD may actually represent a return to the insolation-controlled baseline, and that potentially the*

Bølling-Allerød (B-A) warm interstadial (i.e., GI-1, from 14.642 to 12.846 ka BP), immediately preceding the YD, was the anomaly (Thornalley et al., 2011; Sima et al., 2004). Although an in-depth discussion of the B-A is outside the scope of this study, the B-A may represent an interval with a temporarily invigorated AMOC (an 'overshoot') (Barker et al., 2010), which, after reaching peak strength, began to slow down back towards its glacial state because of the lack of a concomitant rise in insolation (Knorr and Lohmann, 2007; Thornalley et al., 2011). The rate of this slowdown was independent of the final trigger into the YD, and indeed much of the cooling back to the glacial baseline was achieved by ~13 ka BP. Consequently, only a small 'nudge' may have been required to expedite the return to the cold baseline state, consistent with a very sulphur-rich volcanic eruption (occurring every few hundred years) but not necessarily with a rare, high-consequence event. In other words, the B-A may represent the transient anomaly, and the conditions within the YD represented typical near-glacial conditions, obviating the need for an extreme YD triggering mechanism. The cooling after the B-A cannot account for the rapid cooling observed at around 12.9 ka BP clearly visible in the NGRIP δ18O and nitrogen isotope data as well as in numerous other North Atlantic records, indicating that another forcing expedited the final cooling into the YD, which we argue was the LSE."

Comment #6: Related to these ideas is the concept that the BA was an overshoot of the AMOC that occurred during termination of the last glacial, such that its demise was inevitable because climate had not yet shifted to an interglacial state where the on mode of AMOC was stable i.e. the BA should be viewed as the event that needs a trigger, and the YD was simply a return to glacial conditions not requiring a trigger (or only a minor one). This framework for the deglaciation is fairly well established, and I think these concepts ought to be included to ensure that the reader has a more complete and accurate context with which to assess the validity of your hypothesis.

Response #6: Agreed. We have now included text that clarifies the climate context under which the LSE and the YD occurred, as well as appropriate references.

Comment #7: Suggested references (by no means an exhaustive list) that deal with these concepts are: Ganopolski and Rahmstorf 2001, Nature; Schulz et al. 2002 GRL, 10.1029/2001GL013277; Sima et al. 2004, EPSL; Knorr & Lohmann 2007 G^3; Liu et al. 2009, Science, DOI: 10.1126/science.1171041; Barker et al. 2010, Nature Geoscience; Thornalley et al. 2011, Science; Buizert & Schmittner 2015 Paleoceanography; Barker et al., 2015, Nature.

Response #7: We thank the editor for these suggestions. We have now included most of these references into the text. We felt that some of these references were not needed, because the ones we do cite make a sufficiently strong case for the B-A as being particularly anomalous rather than the YD, and we did not want to distract from the main points.

Reviewer #1 Comments and Responses:

Comment #1: Baldini and colleagues present a previously published volcanogenic sulphate record from GISP2 ice cores and compile age estimates for the Laacher See tephra to argue that the LS eruption is recorded in Greenland ice cores as a large sulphate spike positioned approximately at the onset of Greenland Stadial 1 (i.e. Younger Dryas). The authors finally suggest that the LS eruption triggered the YD through a chain of feedbacks resulting from the initial volcanic-induced cooling in the Northern Hemisphere. Even though their hypothesis is tantalizing I find the manuscript excessively speculative and the conclusions very much stretch what can be observed in the reconstructions.

Response #1: We appreciate that this topic is controversial, and the worst-case scenario would be if the manuscript were published without undergoing a rigorous review process. We also thank the reviewer for suggesting that the hypothesis is 'tantalising'; we agree and we hope to convince Reviewer #1 that the conclusions are not overly speculative. We would like to note upfront that we did not 'cherry-pick' the records that were used in the manuscript, rather these were chosen as the records with the most robust chronologies both in absolute terms and with respect to the timing of GS-1 versus the LSE.

Comment #2: I should also point out that the putative link between the LST and a volcanic sulphate spike in GISP2 records was originally suggested by Brauer et al. (1999). In my opinion, the present manuscript doesn't offer anything new but I brief review of the climatic implications of volcanic cooling and far-reaching speculations on the triggers for the YD.

Response #2: Should this manuscript be published, it would open the door to the consideration of a new trigger for the Younger Dryas Event. Of the most recent reviews of the Younger Dryas and its possible causes (e.g., Carlson 2010, *Geology*; Fiedel 2011 *Quaternary International*; Broecker et al., 2010 *Quaternary Science Reviews;* Renssen et al., 2015 *Nature Geoscience*), none even mentions volcanic forcing; we therefore disagree wholeheartedly with the reviewer's statement that this '…manuscript doesn't offer anything new…'. This manuscript presents a very novel and provocative hypothesis, and does not represent 'business-as-usual'. Although there are certainly elements of a review, we are the first paper to discuss the LSE as a YD trigger in any detail, and we are the first to detail the positive ice-AMOC feedback following the LSE within the context of the YDE. We also identify the sulphate spike associated with the Laacher See eruption, using the most recent chronology for the GISP2 ice core. Reviewer #1 is correct that Brauer et al 1999 did consider this same spike as being potentially linked to the LSE, but ultimately they decided that it occurred too close to the YD boundary and concluded that an earlier spike represented the LSE (this is now discussed); so I think it is correct to say that we are the first to attribute the LSE to this particular spike, though we will now discuss the fact that Brauer et al (1999) considered the spike before us.

This manuscript uses previously published data to reach new conclusions, and we therefore feel that this goes considerably above and beyond a 'brief review', and we feel very strongly that we do offer something new. For example, several recent papers incorrectly state that the LSE occurred ~200 years before the start of the YD (e.g., Moore et al 2017; Wolbach et al., 2018a, Wolbach et al, 2018b); this would probably not have happened if our submission had been published.

Comment #3: Assigning the LS eruption to a nearly synchronous large sulphate peak in GISP2 is a flawed assumption until proven otherwise by tephochronological analyses. The authors should consider alternative hypotheses. For instance, volcanic records from Greenland are particularly sensitive to Icelandic eruptions due to the eruptive frequency and proximity (e.g. Abbott and Davies, 2012). Hence, even small-size Icelandic eruptions characterised by moderate sulphate emissions can result in disproportionally large sulphate anomalies in ice core stratigraphies.

Response #3: We agree that tephochronological analyses would confirm our identification, and we have now included this statement in the text. We note that although this is certainly an excellent idea, the lack of tephochronological data did not prevent most other researchers from attributing sulphate spikes to individual eruptions (for example, Brauer et al., 1999, mentioned by the reviewer in Comment #2, Svensson et al., 2013 Climate of the Past, and many others). We have also provided ideas for confirming our hypothesis in general in the conclusion.

Comment #4: 2. I think the paper would greatly benefit from exploring some of the mechanisms the authors discuss (e.g., southward wind shifts, AMOC decline, sea ice expansion, etc.) by looking into climate model output and/or historical reanalyses data sets. The authors present output data from MAECHAM4 simulations but they don't provide any additional analysis of the atmospheric parameters in the model. CMIP historical climate simulations and PMIP last-millennium simulations offer valuable model data to examine the role of volcanic eruptions on the coupled atmosphere-ocean system. Model output based on volcanically-forced transient simulations with earth System Model (MPI-ESM) (Jungclaus et al., 2010) (available online) may also be useful.

Response #4: We now include a revised and extended discussion of relevant, previously published model outputs over the last two millennia. The climate and AMOC response following volcanic eruptions is different in each of the models; this is now discussed:

"*Our hypothesis that the YD was triggered by the LSE and amplified by a positive feedback is further supported by modelling results suggesting that a combination of a moderate negative radiative cooling, AMOC weakening, and altered atmospheric circulation best explain the YD (Renssen et al., 2015). AMOC consists of both* *thermohaline and wind-driven components, and atmospheric circulation changes can therefore dramatically affect oceanic advection of warm water to the North Atlantic. Recent modelling suggests that reduced wind stress can immediately weaken AMOC, encouraging southward sea ice expansion and promoting cooling (Yang et al., 2016), illustrating a potential amplification mechanism following an initial aerosol-induced atmospheric circulation shift. Twentieth Century instrumental measurements further support this by demonstrating that westerly winds strength over the North Atlantic partially modulates AMOC (Delworth et al., 2016).*

Initiation of the positive feedback requires volcanic aerosols to remain in the atmosphere for at least one summer season. Evidence based on the seasonal development of vegetation covered by the LST suggests that the LSE occurred during late spring or early summer (Schmincke et al., 1999), and varve studies similarly suggest a late spring or early summer eruption (Merkt and Muller, 1999). Available evidence therefore suggests that the eruption occurred just prior to maximum summer insolation values, maximising the potential scattering effects of the volcanogenic sulphate aerosols. Even if it were a winter eruption, for historical eruptions similar in magnitude to the Laacher See eruption, aerosols remained in the atmosphere longer than one year, regardless of the eruption's latitude. For example, aerosols remained in the atmosphere for ~three years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which probably released considerably less SO2 than the LSE (Figure 2). Measurable quantities of aerosols remained in the atmosphere for approximately three years even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected only 2.1 Mt SO2 (Baales et al., 2002; Pitari et al., 2016) and erupted laterally (Eychenne et al., 2015). In short, the LSE eruption probably occurred during the late spring or early summer, but even if the eruption were a winter eruption, the LSE's aerosols would have certainly persisted over at least the following summer, with the potential to catalyse the positive feedback we invoke."

Comment #5: I think some of the arguments proposed here (as well as the records presented in Figure 3) have conveniently been picked to craft a story where the LS eruption stands out as the INITIAL cooling that triggered the YD. This does not faithfully reflect the state of the knowledge around the dynamics that took place prior to and at the onset of the YD. Several reconstructions show that a gradual but substantial cooling across Northern Europe and in the Nordic Seas preceded the start of GS-1. Pollen records from the British Isles indicate cooling as early as 13,200 years BP (Walker et al., 2012). A drop in air temperatures a few centuries before GS-1 has also been recorded in chironomid-based temperature records from Norway (Bakke et al., 2009; see supplementary information), Sweden (Muschitiello et al., 2015), and the Netherlands (Heiri et al., 2007). Similarly, sea surface temperature records from the Norwegian Sea indicate a rapid cooling approaching YD values as early as 13,500 years BP (Bakke et al., 2009) with temperature values dropping by ca. Well-dated paleoceanographic records from the coast of Norway also show a progressive aging of surface waters starting at ca. 13,200 years BP (Bondevik et al., 2006), which is suggestive of a slowdown of surface-water circulation and reduced advection of warm subtropical waters prior to the start of GS-1. I therefore believe that the authors should do a better job in contextualizing the LS eruption in the regional climate picture since proxy reconstructions from Central Europe (e.g. Grafenstein, 1999; Rach et al., 2014) are evidently not fully representative of large-scale North Atlantic climate. In particular, the records mentioned above clearly suggest that cooling and climate deterioration was long underway before the start of the YD, which implies that the cooling associated with the LS eruption cannot be the trigger for the YD.

Response #5: We appreciate the very thorough suggestions for other records to consider, and here we provide reasons why the records are, or are not, relevant to the Laacher See hypothesis. Again, we assure the reviewer that we have not knowingly excluded records that contradict our hypothesis. Rather, the records chosen are those with the most robust chronologies both in absolute terms and with respect to the timing of GS-1 versus the LSE. All of the records used contain both a high resolution regional temperature proxy record and either the Laacher See Tephra directly, or an excellent layer-counted chronology.

Muschitiello et al., 2015 Nature Communications: We do not doubt that meltwater pulses did occur and undoubtedly affected climate during the last deglaciation. In fact, we suggest that the YD was terminated by a Southern Hemisphere meltwater pulse that triggered long-term warming. Muschitiello et al. argue that meltwater forcing affected climate from 13.1 to 12.880 ka BP. We do not argue against this, but nothing in this paper contradicts our manuscript. In fact we note that, once again, a pronounced climate shift occurs coincident with the Laacher See eruption at 12.880 ka BP. The large inflection point in Figure 4 (Panel d and elsewhere) is indistinguishable from the date of the eruption. It is currently difficult to disentangle whether the forcing was a meltwater pulse from the Fennoscandian Ice Sheet (or elsewhere), a bolide impact, or the Laacher See eruption, and we hope that our research will promote future debate assessing the pros and cons of each hypothesis.

We also note that it is conceivable that the Fennoscandian Ice Sheet was melting from 13.1 to 12.88 ka BP due to rising insolation, releasing meltwater. The end of this meltwater pulse coincides perfectly with the cooling (GS-1) that we argue was triggered by the LSE; it is therefore reasonable that post-eruptive cooling, combined with a positive feedback, could temporarily reverse Fennoscandian Ice Sheet melting. We also note that Muschitiello et al. state that summer temperature dropped by 2 degrees at 12.883±0.035 ka BP, "suggesting substantially drier and colder summer conditions." It is conceivable that this is reflecting the direct aerosol effects of the LSE.

Heiri et al. 2007: The paper presents an interesting chironomid-based temperature record of the Younger Dryas from the Netherlands. Unfortunately, it is too low resolution to be particularly useful to this manuscript. The dating is also not as high precision as the records that we have chosen, though we note that the decrease in July temperature starts just after the eruption based on their chronology. Still, we choose not to include this record and others like it because of the low resolution and more uncertain chronology.

Walker et al 2012: Unfortunately the low resolution and ambiguous results make this paper a low priority for inclusion. There are hundreds of Younger Dryas climate reconstructions globally, and including all of them is simply not possible. Pollen reconstructions in particular are problematic due to the generally lower resolution of the datasets, the often less well-constrained chronologies, and the local nature of the proxy. We do not see how this paper provides a significant challenge to either a LSE, a bolide impact, or a meltwater trigger for GS-1 and the YD.

Bondevik et al., 2006: This is quite an interesting paper that we somehow had missed previously, and we thank the reviewer for bringing it to our attention. Their Figure 3 is particularly striking, and shows a pronounced inflection point in the radiocarbon concentrations precisely at the Laacher See eruption (Panels B, C, and D). They state that *'A high reservoir age during the YD could be explained by a combination of increased sea-ice cover and reduced advection of surface water to the North Atlantic'*, both of which are entirely consistent with our proposed positive feedback mechanism. We have included this reference to the radiocarbon data within the context of a more substantial discussion of the positive feedback.

Bakke et al., 2009: The correlations between the data presented in this paper and the Laacher See eruption are remarkably strong. In particular, Supplemental Figures S5 (noting of course that their data is in years b2k) and S7 show that the largest inflection point is coincident with the eruption. We thank the reviewer for bringing this supplemental material to our attention.

Specific comments:

L30: An alternative route has been proposed involving the drainage of the Baltic Ice Lake, the timing of which precisely coincide with the start of GS-1 (Muschitiello et al., 2016).

Response: This is now discussed.

L42-47: Without entering into the discussion on the credibility of the "impact" theory, I think that the evidence is still undermined by the poor chronological accuracy of the platinum/iridium anomaly.

Response: We now have an entire new section of almost 1,000 words discussing the other leading theories in more detail.

L54: What stratigraphic frameworks? Please specify and provide reference.

Response: We have now provided more context regarding this statement.

L57-58: ": : :consistent with the LS eruption: : :" Please add here "as recorded in varved and 14C dated lake records".

Response: We have reworded this sentence.

L148: Please provide reference here on the impact of volcanic forcing on AMOC (e.g. Otterå et al., 2010).

Response: We have now added this reference as well as several others in a more detailed discussion of the feedback.

L162: Please specify what model. This is not clear.
Response: This has been rephrased to be clearer.

L176-192: This section is all very speculative. Please see my comment on the possibility of examining climate model output to support these claims.
Response: We have added a substantially strengthened section regarding relevant published climate model results.

L181: None of the studies cited here present direct evidence of sea ice changes (only indirect and mainly based on terrestrial reconstructions). Please consider referring to marine reconstructions (e.g. Cabedo-Sanz et al., 2013).
Response: We have now added this reference.

L223: I'm not convinced that the data in Figure 5a and b follow a Gaussian distribution. Rather, the frequency distributions seem skewed. Also the authors cannot claim that "a Gaussian distribution exist" when they fit a Gaussian-best-fit model to their data. They should use a resampling/bootstrap method to draw from their empirical distribution and only then establish its shape.
Response: We have changed the wording in the main text. We have left the wording the same in the figure caption, because the grey-filled curve is a Gaussian distribution, and this describes the shape of the data according to the software used.

L219-230: I suggest estimating the frequency of Greenland cooling relative to the number of volcanic events occurring at the end of the preceding interstadial. This should somewhat inform on the potential link between volcanisms and the onset of stadial cooling.
Response: Previously published research (e.g., Zielinski et al., 1997; Sternai et al., 2016) has already made the connection between increased frequency of volcanism and deglaciation, and this is mentioned a couple of times in the text. The number of volcanic events relative to the number of Greenland cooling events is discussed in detail in one of our prior publications (Baldini et al., 2015) and already mentioned in this manuscript.

L246: The rate of cooling (as seen in the d18O ice-core stratigraphies) is similar among most of the stadials, not exclusively between GS-1 and GS-20.
Response: Agreed, and we would argue that the cause is similar. Baldini et al. 2015 showed that every well-dated, large volcanic eruption over the period 30-80 ka BP is within dating errors of a stadial. The post-eruptive positive feedback is likely to be identical between GS-1 (Laacher See), GS-20 (Toba), and many (most?) other stadials – GS-12 (Opala), GS-9 (Campi Flegrei), etc. Unfortunately most eruptions are still very poorly dated, but

we would venture that every sulphur-rich magnitude 6 (or above) eruption occurring during intermediate ice volume conditions resulted in the positive feedback. We have added a substantial amount of text that will hopefully make this clearer.

L247-249: The initial cooling can be tested using ice-core (e.g. d15N) and other proxy based temperature reconstructions and compare the magnitude of the temperature change with that associated with large historical eruptions.
Response: We have added statements discussing Greenland d15N. We are unsure of the added value in looking for the initial cooling - there is no doubt that the eruption happened, and no doubt that it contained considerably more sulphur than the climatologically important Pinatubo eruption. So it almost certainly did result in cooling, but maximum cooling probably persisted for one year (more subdued cooling would have lasted for ~3 years). This would be difficult to detect in an ice core, and even more difficult to attribute to an eruption. Even if detectable, the cooling could also be ascribed to a meltwater pulse or a bolide impact. Regardless, existing d15N reconstructions (Buizert et al 2014, Science) are fully consistent with the manuscript, and we have included these references in the discussion.

L253: Is this claim based on ice-core d18O profiles? I don't see any substantial warming in Greenland during the second half of GS-1. d18O of ice can be misleading due to changes in moisture source of precipitation. Before making this claim I would check the ice-core temperature records (δ^{18}O diffusion or d15N). As far as I can tell from the d18O data both GS-1 and GS-20 in Greenland were characterised by cold conditions throughout the stadial.
Response: There is abundant evidence that the maximum cooling during GS-1 occurred at around 12.65 ka BP, and that this was followed by moderate gradual warming. This is apparent not only in NGRIP and GISP2 δ^{18}O, La Garma Cave (Spain) δ^{18}O, Chauvet Cave δ^{18}O (France), Lake Ammersee δ^{18}O, etc., but also in ice core nitrogen isotope ratios and deuterium records. We have added add some references to this statement to clarify.

L258-260: Again, this is extremely speculative and I don't think there is any evidence supporting this claim.
Response: Please see our response to the point above. We do not see how this is 'extremely speculative' when there is abundant evidence supporting the claim. We have now added more references to further support the statement.

L275-284: The magnitude of the spike does not necessarily scale linearly with the magnitude of the eruption and could solely depend on the proximity of the volcanic source or the atmospheric circulation pattern.

Response: This is true, and we noted this in our previous submission when we state that the large spike preceding our candidate LSE spike is a small Icelandic eruption of Hekla. We have added some extra text to the manuscript to ensure that readers are clear on this point.

L290: Age uncertainties for the LST and the sulphate spike should be reported on their respective time scales (i.e. GICC05, IntCal13, MFM varve time scale, etc.). In addition, time scale offsets between the 14C time scale and GICC05 should be accounted when comparing radiocarbon-based and ice-core ages (e.g. Muscheler et al., 2014).

Response: The timing of the sulphate spike relative to the LSE is based on layer counting in the Meerfelder Maar core from the LST to the Vedde Ash, and then layer counting from the Vedde Ash to the position where the LSE should be (and where we find a sulphate spike). We will clarify this in the section where we discuss the sulphate spike. Muscheler et al., 2014, note that *"…there is no evidence for any significant difference between the GICC05 ice-core and 14C time scales at around 13,000 yr BP."* and we now mention this.

L291: Please see my previous comment. Cooling in the North Atlantic started a few centuries before the onset of GS-1.

Response: It is true that cooling following the B-A warm interval had been occurring for some time prior to 12.9 ka BP. However, there is a clear increase in the rate of cooling at 12.9 ka BP, as discussed in innumerable publications on the YD. We now include a discussion on the B-A.

L305-306: As I mentioned above, it would be helpful to look into the frequency of stadials relative to the number of volcanic eruptions during the preceding interstadials.

Response: Please see our response to the reviewer's same comment above (comment on L219). This has already been previously, and is discussed and cited in the manuscript.

Reviewer #2 Comments and Responses:

Comment #1: I am not convinced by this paper, and so recommend rejection or major revisions. Perhaps the timing of the eruption is close to indicators of cooling (not my specialty), but the hand-waving arguments about why this eruption caused cooling and larger ones did not are not convincing.

Response #1: We are sorry to read that Reviewer #2 feels that our arguments are overly speculative. We feel that perhaps the reviewer has missed some key points, which probably reflects that these were poorly communicated on our part. We hope that the revised submission as well as our responses will help better communicate our hypothesis, and we welcome the opportunity to improve the manuscript.

Regarding the last part of Reviewer #2's comment, we are unclear as to which eruptions the reviewer is referring to when Reviewer #2 mentions '…larger ones did not…'? We state:

'…it is in fact the only known sulphur-rich high latitude eruption coinciding with the most sensitive ice volume conditions during the last deglaciation. '

and devote an entire section to other eruptions (Section 3.6), so the other large eruptions that the reviewer is referring to is not clear. We have published previous research indicating that there is a strong statistical significance between large Northern Hemisphere eruptions and long-lasting climate change (Baldini et al., 2015, Scientific Reports) and there is now evidence that Toba also triggered long-lasting climate change (discussed below, in our responses to Comment #7). So there is strong evidence that other larger eruptions did cause cooling (Toba and the several others discussed in Baldini et al., 2015), and we argue that the Laacher See eruption occurred during a particularly sensitive climatological transition, so should also be expected to trigger cooling. We now also include a lengthy section discussing volcanic impacts on climate over the last millennium, and an enhanced discussion regarding the amount of sulphur in the eruption, which was substantial. Part of that enhanced discussion is pasted below:

"*It is worth highlighting that a similar mechanism may have also contributed to Little Ice Age cooling (Miller et al., 2012), with research suggesting that a coupled sea ice/AMOC mechanism could extend the cooling effects of volcanic aerosols by over 100 years during the Little Ice Age (Zhong et al., 2011). This perspective is supported by modelling results suggesting that a large volcanic forcing is required to explain Little Ice Age cooling (Slawinska and Robock, 2017). Lehner et al. (2013) identify a sea ice/AMOC/atmospheric feedback that amplified an initial negative radiative forcing to produce the temperature pattern characterising the Little Ice Age. Rahmstorf et al. (2015) argue that no identifiable change in AMOC occurred during the Little Ice Age, suggesting that the drivers were related to sea ice and atmospheric rather than oceanic, although this requires further research to confirm. Large volcanic eruptions in 536, 540, and 547 AD are hypothesised to have triggered a coupled sea ice/AMOC feedback that led to an extended cold period (Buntgen et al., 2016). Recent research also highlights the* *possibility that volcanism followed by a coupled sea ice/ocean circulation positive feedback triggered hemispheric-wide centennial to millennial-scale variability during the Holocene (Kobashi et al., 2017). If a feedback was active following volcanic eruptions during the 6th Century, the Little Ice Age, and the Holocene, the intermediate ice volume and transitional climate characteristic of the last deglaciation should have amplified their effects. This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated "Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval". Our results are entirely consistent with this perspective, and here we highlight a sulphur-rich volcanic eruption whose timing coincided with the onset of YD-related cooling. However, despite increasingly tangible evidence that eruptions can affect AMOC strength and sea ice extent, the exact nature of any positive feedback is still unclear. Future research should prioritize the identification and characterisation of this elusive, but potentially commonplace, feedback that amplifies otherwise subtle NH temperature shifts."*

Comment #2: Indeed more work is required, including climate model simulations that include all the relevant processes.

Response #2: We agree that climate model simulations would benefit the research, but we have decided not to include them for three reasons. i) Neither the meltwater forcing nor the bolide impact hypotheses used climate model simulations to support the initial hypotheses initially, yet both hypotheses led to fruitful discussions and future elaborations, including climate model research. We therefore feel that climate model simulations would make for interesting future work, and indeed we are looking into this ourselves. But we do not feel that they are required for this current submission. ii) There is a good chance that different climate models would return different answers, in which case modelling is better left for future researchers who can devote considerable attention towards developing robust and replicable model outputs. iii) We may not know the details of all the relevant feedbacks, and therefore any model might be incomplete. This issue is actually brought up by the reviewer in their own phraseology: "…climate model simulations that include *all the relevant processes*." The issue is that we may not know all the relevant processes. For example, if we knew that there was in fact a pronounced positive feedback following Northern Hemisphere eruptions during intermediate ice volume conditions, we would know that this feedback was responsible for the Younger Dryas Event as well as other Greenland stadial events. Possibly the reason that we do not know what forced these events is that we do not know about the relevant process, and if that is the case, modelling could be misleading. This is point is further illustrated by a recent paper by Diallo et al., 2017 GRL, that states *"Thus, climate model simulations need to realistically take into account the effect of volcanic eruptions, including the minor eruptions after 2008, for a reliable reproduction of observed stratospheric circulation changes."* If ambiguous modelling results, and the incorporation of unrealistic eruptions effects into models, are demonstrably an issue over the last decade, they will be even more of an issue during the less-well understood YD interval where a key feedback may remain unquantified. For these reasons, we feel strongly that modelling is best left for future work where intercomparison between different models is possible. We hope that this paper, if published, would encourage modellers to consider the mechanisms and feedbacks proposed, and include these in future models.

Comment #3: Even if the timing was close, there is no proof that this was not just a coincidence. The authors claim that the climate system was particularly sensitive to volcanic forcing at the time, but this is just speculation. Where are the model results to show this?

Response #3: Both of the other leading hypotheses for a Younger Dryas trigger also rely on the coincidence of a trigger with the advent of cooling. For example, the meltwater pulse hypothesis relies on the timing of a meltwater pulse with the start of the Younger Dryas. However, unlike the Laacher See hypothesis, there is little agreement within the community regarding the source of that meltwater or in fact whether it even occurred simultaneously as the start of GS-1. The bolide impact hypothesis also relies on the coincidence of evidence for a meteor airburst or impact with the Younger Dryas initiation, but whether or not this even occurred is extremely controversial. The Laacher See hypothesis does rely on coincidence, but the event itself is much more clearly expressed than the other leading hypotheses. In fact, it is the only hypothesis that features a trigger that is universally accepted as actually having happened. We have now included a lengthy (almost 1,000 words) new section (‘3.7 Compatibility with other hypotheses’) to better discuss how the Laacher See hypothesis is competitive relative to the other leading proposed triggers.

Please see our response to why we choose not to include models above (Response #2).

Finally, we are not the first to suggest that the climate system is particularly sensitive to volcanic forcing during climatological transitions, and this is not ‘speculation’. It is well established that millennial-scale climate change was most sensitive to a forcing during intermediate ice volume conditions, and we simply propose that that forcing was volcanism. Zielinski et al. (1996) noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self 1992 (*Nature*) stated that ‘Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval.’ Our research confirms and builds on this earlier work, and identifies a volcanic eruption potentially responsible for the YD. We have included the following text:

“This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated ‘Volcanic aerosols may also

contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval.'. Our results are entirely consistent with this perspective, and here we identify the volcanic eruption responsible for the YD."

Comment #4: In fact, in Fig. 4 there are two larger eruptions during the same period. Why did only Laacher See produce cooling? They claim in the Fig. 4 caption that the Hekla eruption was more proximal, and therefore should be discounted, but the way it works is that Icelandic eruptions into the westerlies have to go around the world before the acid snow deposits on Greenland, and so there is no reason to think that it would have a smaller climate impact than Laacher See.

Response #4: The volcanological information regarding the size of the Hekla eruption is from published sources (Muschiatello et al., 2017, Nature Communications; Mortensen et al 2005, Journal of Quaternary Science), so we are simply referring to previously published research when we refer to the fact that Hekla was substantially smaller than the Laacher See eruption, and that it appears in the GISP2 ice core. Additionally, the reviewer is incorrect about Icelandic eruptions, and it is well-established that sulphate from even small Icelandic eruptions appears in Greenland ice cores (Muschiatello et al., 2017, Nature Communications; Abbott and Davies, 2012, Earth Science Reviews; Abbott et al., 2012, QSR). Muschiatello et al. (2017) state: "Icelandic volcanoes remain the dominant source of volcanogenic aerosols in Greenland ice cores due to their relative proximity and high eruptive frequency". The other eruption we mention in the Figure 4 caption is the Nevado de Toluca eruption; we refer the reviewer to the text already in our manuscript that explains why its climate expression may not be as clear:

'The eruption was approximately the same size as the LSE, so the lack of climate cooling may reflect a different climate response due to the eruption's latitude, which caused a more even distribution of aerosols across both hemispheres, or a lower sulphur load. The 12.6 ka BP sulphate spike is associated with a short but dramatic cooling; therefore the lack of long-term cooling may simply reflect the fact that temperatures had already reached the lowest values possible under the insolation and carbon dioxide baseline conditions characteristic of that time.'

Comment #5: Even the size of the eruption is speculation, and the authors mix mass of SO2 with that of elemental Sulphur with that of stratospheric aerosol. What do they claim actually was the stratospheric loading for this eruption? And each time you talk about mass, please convert it to the same chemical so it can be compared.

Response #5: We thank the reviewer for picking up on this. This partially stems from an ambiguity in a previously published paper that we used as a reference. We have now converted everything to the same chemical, except where the context requires us to discuss a different one.

Comment #6: The title is confusing. Why is it "re-evaluating?" There is no initial evaluation that is addressed in the abstract or in the paper.

Response #6: This issue was also brought up by another comment by another reader of the manuscript. The Laacher See eruption was mentioned briefly (just a couple of sentences) in the late 1980s as a trigger for the YDE, before it was discarded due to evidence incorrectly showing that it occurred too early. However, the most recent lake core and ice core data indicate that the beginning of YD cooling (the start of GS-1) occurred synchronously with the LSE, so we feel that 'Re-evaluating' is the correct word to use here. We stated in the introduction of our originally submitted manuscript:

"Instead, we re-introduce and provide new support for the hypothesis that the YD was triggered by the ~12.9 ka BP eruption of the Laacher See volcano, located in the East Eifel Volcanic Field (Germany). Early research considered the eruption as a possible causative mechanism for the YD (Berger, 1990). However, the concept was dismissed because lacustrine evidence across central Europe appeared to indicate that the YD's clearest expression appeared ~200 years after the Laacher See Tephra within the same sediments (e.g., Brauer et al., 2008; Brauer et al., 1999; Hajdas et al., 1995)."

We were not the first to suggest the eruption as a trigger, although we are 're-evaluating' the eruption's climatological consequences in a modern context and presenting far and away the most developed hypothesis implicating the eruption as the YD trigger. Still, because two reviewers raise this same issue, we have now included the following text to better explain why we chose to use this term in the title:

"*Here we summarise but do not argue extensively for or against any of these established hypotheses. Instead, we investigate the hypothesis that the YD was triggered by the ~12.9 ka BP eruption of the Laacher See volcano, located in the East Eifel Volcanic Field (Germany). Earlier research briefly alluded to the eruption as a possible causative mechanism for the YD (e.g., Berger, 1990; Bogaard et al., 1989). However, because the meltwater pulse hypothesis was already popularised, and because the effects of volcanic eruptions on climate would escape detailed quantification until after the 1991 Pinatubo eruption, the ~12.9 ka BP Laacher See eruption as a YD trigger never gained traction. Importantly, the concept was effectively dismissed after lacustrine evidence across central Europe appeared to indicate that the YD's clearest expression appeared ~200 years after the Laacher See Tephra within the same sediments (e.g., Brauer et al., 2008; Brauer et al., 1999a; Hajdas et al., 1995). For example, Schmincke et al. (1999) state that "The Younger Dryas cooling period clearly was not triggered by LSE as formerly thought because it started ca. 180 years after the eruption", reflecting the accepted sequence of* *events at that time. However, the identification of the Vedde Ash chronostratigraphic unit within Meerfelder Maar (Germany) lake sediments has permitted improved correlation with Greenland ice core records, which also contain the same ash (Lane et al., 2013a). This revised chronological framework now strongly suggests that the 12.880 ka BP Laacher See eruption was in fact synchronous with cooling associated with the YD onset (i.e., the most recent abrupt Greenland millennial-scale cooling event, Greenland Stadial-1; 'GS-1'), but preceded major atmospheric circulation shifts over central Europe (Rach et al., 2014).*

Furthermore, we utilise ion data from the Greenland ice core GISP2 (Zielinski et al., 1997) on the most recent chronological model for the core (the GICC05modelext chronology (Seierstad et al., 2014)) to identify a large volcanogenic sulphate spike whose timing coincides with both the Laacher See eruption and the initiation of GS-1 related cooling. We argue that the initial, short-lived volcanogenic aerosol cooling triggered a sea-ice/AMOC positive feedback that caused both basin-wide cooling and the dynamical climate shifts most closely associated with the YD. Therefore we are 're-evaluating' the Laacher See eruption's role in triggering the YD, building on research that initially briefly mentioned the eruption as a causative mechanism for the YD, and later research that dismissed the original version of the concept."

Comment #7: Since the Laacher See eruption was high latitude, we would expect that for the same stratospheric loading, it would have much less of a climate impact than an equivalent tropical eruption, since the atmospheric lifetime would be much shorter, and there is less insolation at high latitudes. When you compare to Toba, this must be addressed.

Response #7: We have added the following text to discuss this:

"The residence time of aerosols within the atmosphere is not critical within the context of this model provided the positive feedback is activated, and a sufficiently high aerosol-related cooling over only one summer and one hemisphere could suffice. The strength of the feedback may also depend on the amount of hemispheric temperature asymmetry caused by the eruption. Consequently, the high latitude LSE may actually have induced a stronger hemispheric temperature asymmetry than the low latitude Toba eruption, although the Toba eruption would have resulted in considerably more overall cooling. The long-term (e.g., hundreds to thousands of years) climate response will depend on the climate background conditions; if a NH eruption occurs during an orbitally-induced cooling trend (as may have been the case for Toba), the eruption will catalyse cooling towards the insolation-mediated baseline."

Comment #8: And if the eruption was in the fall or winter, most of the aerosol would have fallen out of the stratosphere before the Sun comes up the next summer and there would be minimal impact on climate.

Response #8: For historical eruptions similar in size to the Laacher See eruption, aerosols have remained in the atmosphere for much longer than one year. For example, aerosols remained in the atmosphere for ~3 years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which is estimated to have released considerably less sulphur than the LSE (Sheng et al., 2015; Baales et al., 2002). Aerosols remained in the atmosphere for more than one year even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected an order of magnitude less sulphur into the atmosphere than the LSE (Pitari et al., 2016; Baales et al., 2002) and erupted laterally (Eychenne et al., 2015, JGR-Solid Earth). Furthermore the eruption almost certainly occurred in the late spring. We have added the following text to discuss this in the manuscript:

"Initiation of the positive feedback requires volcanic aerosols to remain in the atmosphere for at least one summer season. Evidence based on the seasonal development of vegetation covered by the LST suggests that the LSE occurred during late spring or early summer (Schmincke et al., 1999), and varve studies similarly suggest a late spring/early summer eruption (Merkt and Muller, 1999). Available evidence therefore suggests that the eruption occurred just prior to maximum summer insolation values, maximising the potential scattering effects of the volcanogenic sulphate aerosols. Even if it were a winter eruption, for historical eruptions similar in size to the Laacher See eruption, aerosols have remained in the atmosphere far longer than one year, regardless of the eruption's latitude. For example, aerosols remained in the atmosphere for ~three years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which is estimated to have released approximately nine times less SO2 than the LSE (~15-20 Mt versus 150 Mt) (Sheng et al., 2015; Baales et al., 2002). Aerosols remained in the atmosphere for approximately three years even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected almost 100x less SO2 into the atmosphere than the LSE (2.1 Mt versus 150 Mt) (Baales et al., 2002; Pitari et al., 2016) and erupted laterally (Eychenne et al., 2015). In short, the LSE eruption probably occurred during the late spring or early summer, but even if the eruption were a winter eruption, the LSE's aerosols would have certainly persisted over at least the following summer, with the potential to catalyse the positive feedback we invoke."

Comment #9: The paper is replete with undefined acronyms, making it very confusing. All acronyms have to be defined the first time they are used. For example, what is LST? It is never defined. Is it LSE and a typo? What are TOMS, NGRIP, GISP2, GICC05modelext, ITCZ, GS-20, GI-19 (in Fig. 6), ...? Please keep in mind that there will be readers not from your specific discipline, and so jargon needs to be defined. GS-1 is finally defined long after it is used, but the authors still never say what Greenland Stadial 1 is. What is a stadial? Why does Greenland have one? How many does it have?

Response #9: The reviewer is quite correct here, and we apologise for not defining these terms. We have gone through the manuscript and defined all acronyms and terms which might be confusing for a non-specialist. We have now also mentioned the total number of stadials as well at a relevant point in the manuscript.

Comment #10: The paper talks about magnitudes for volcanic eruptions, but never says what the scale is. Magnitude of what? If not of sulphur injection, then what is the point? And where do the data come from? There are no references to that.

Response #10: 'Magnitude' is a common term in volcanology that refers to the amount of tephra and lava erupted. It is very difficult to know for sure how much sulphur was in eruptions recorded in the geological past due to the fact that much of the sulphur existed in a volatile phase and is not preserved in the rock record. In general magnitude and sulphur concentrations are well correlated (Oppenheimer et al., 2003), and therefore magnitude is therefore often used as a surrogate for sulphur concentrations. All the available evidence suggests that if the LSE deviates from this trend, it is anomalously enriched in sulphur than expected. We have added the following text to the manuscript:

"Both Pinatubo and the LSE were Magnitude 6 (M6) eruptions, where 'magnitude' is a measure of eruption size referring to the amount of material erupted (Deligne et al., 2010) on a logarithmic scale. However, the cooling effects of a volcanic eruption are controlled by the amount of sulphur released, and not necessarily the eruption size (Rampino and Self, 1982). In general, magnitude and erupted sulphur amounts are well correlated (Oppenheimer, 2003), and therefore magnitude is often used as a surrogate for sulphur yield. All the available evidence suggests that if the LSE deviates from this trend, it was anomalously enriched in sulphur relative to its magnitude (Baales et al., 2002; Scaillet et al., 2004), and that it therefore should have produced significant NH cooling."

Comment #11: As for the Toba eruption, the paper is missing key references on the climate impact. Robock et al. (2009) found a larger short-term impact, but no long-term effect. Timmreck et al. (2010) claim that it would have had a small impact, as the particles would have grown and had a smaller impact per unit mass.

Robock, A., C. M. Ammann, L. Oman, D. Shindell, S. Levis, and G. Stenchikov, 2009: Did the Toba volcanic eruption of _74 ka B.P. produce widespread glaciation? J. Geophys. Res., 114, D10107, doi:10.1029/2008JD011652. Timmreck, C., et al., 2010: Aerosol size confines climate response to volcanic supereruptions. Geophys. Res. Lett., 37, L24705, doi:10.1029/2010GL045464.

Response #11: We thank the reviewer for these suggestions, and we have now included these references as well as an enhanced discussion.

Comment #12: In any case, I find the Haslam and Petraglia (2010) Figure 1 very convincing that it got cold before the eruption. By the way, that reference is missing from the reference list. Why does the timing of the Toba eruption in Fig. 6 here differ from that in Fig. 1 of Haslam and Petraglia (2010)? Which is correct, and why?

Response #12: As we note in our previous submission's text, the timing of the Toba eruption S spike within the ice cores is based on Svensson et al., 2013, Climate of the Past, which uses a very thorough analysis of both Arctic and Antarctic ice core records. This represents the most up-to-date assessment of the timing of the Toba eruption relative to Greenland climate change, so we use this, in agreement with other recent publications (e.g., Polyak et al, 2017, Geology). The Haslam and Petraglia 2010 paper precedes the Svensson et al. 2013 analysis, and uses an older chronology. So our assessment is correct, and Haslam and Petraglia 2010 is incorrect, at least in terms of reflecting the most recently accepted chronology. Because the Haslam and Petraglia 2010 paper is demonstrably out-of-date (through no fault of their own – the paper preceded the chronological revisions we used), it is not worth discussing at length in this manuscript.

Comment #13: The paper ignores all the work that has shown that the 1257 Samalas eruption caused the Little Ice Age (Zhong et al., 2011; Miller et al., 2012; Slawinska and Robock, 2017). What does this tell us about the claim that a much smaller eruption of Laacher See caused a much larger climate response?

Miller, G. H., Á. Geirsdóttir, Y. Zhong, D. J. Larsen, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, K. A. Refsnider, S. J. Lehman, J. R. Southon, Ch. Anderson, H. Björnsson, and T. Thordarson, 2012: Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean feedbacks. Geophys. Res. Lett., 39, L02708, doi:10.1029/2011GL050168. Slawinska, J., and A. Robock, 2017: Impact of volcanic eruptions on decadal to centennial fluctuations of Arctic sea ice extent during the last millennium and on initiation of the Little Ice Age. J. Climate, doi:10.1175/JCLI-D-16-0498. http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-16-0498.1

Zhong, Y., G. H. Miller, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, D. P. Schneider, and A. Geirsdottir, 2011: Centennial-scale climate change from decadally-paced explosive volcanism: a coupled sea ice-ocean mechanism. Clim. Dyn., 37, 2373-2387.

Response #13: We thank the reviewer for flagging this up; we have now included these suggested references, as well as some others as well. As for the second part of the comment, we refer the reviewer to our response to their Comment #7 (i.e., the size of the feedback is dependent on the background conditions, not the size of the eruption, provided the eruption is sufficiently large to trigger ice growth and interrupt ocean circulation). We have included a substantial amount of new text in the revised submission:

'A similar mechanism may have also contributed to Little Ice Age cooling (Miller et al., 2012), with recent research suggesting that a coupled sea ice/AMOC mechanism could extend the cooling effects of volcanic aerosols by over 100 years during the Little Ice Age (Zhong et al., 2011). This perspective is supported by modelling results suggesting that a large volcanic forcing is required to explain Little Ice Age cooling (Slawinska and Robock, 2017). Lehner et al. (2013) identify a sea ice/AMOC/atmospheric feedback that amplified an initial negative radiative forcing to produce the temperature pattern characterising the Little Ice Age. Similarly, large volcanic eruptions in 536, 540, and 547 AD are hypothesised to have triggered a coupled sea ice/AMOC feedback that led to an extended cold period (Buntgen et al., 2016). Recent research also highlights the possibility that volcanism followed by a coupled sea ice/ocean circulation positive feedback triggered hemispheric-wide centennial to millennial-scale variability during the Holocene (Kobashi et al., 2017). If a sea ice/AMOC feedback was active following volcanic eruptions during the 6th Century, the Little Ice Age, and the Holocene, the intermediate ice volume and transitional climate characteristic of the last deglaciation should have amplified their effects. This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated "Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval". Our results are entirely consistent with this perspective, and here we highlight a candidate volcanic eruption whose timing coincided with the onset of YD-related cooling. However, despite increasingly tangible evidence that eruptions can affect AMOC strength and sea ice extent, the exact nature of any positive feedback is still unclear. Future research should prioritize the identification and characterisation of this elusive, but potentially commonplace, feedback that amplifies otherwise subtle NH temperature shifts.'

Comment #14: It would have been nice to have used hanging indents or additional spacing for the reference list to make it easier for the reader to find each paper in the list. In addition, there are another 35 comments in the attached annotated manuscript that need to be addressed.

Response #14: We agree with the reviewer, and have added a space between the references for increased legibility. We have addressed all of the comments contained in the annotated manuscript.

Reviewer #3 Comments and Responses:

Comment #1: The manuscript "Re-evaluating the link between the Laacher See volcanic eruption and the Younger Dryas" by Baldini et al proposed Laacher See Eruption (LSE) as a potential trigger of the YD cold interval during the last deglaciation. The manuscript is good written and easy to follow.

Response #1: Thank you for these positive comments, which are much appreciated.

Comment #2: The authors argued that radiative effect of the LSE could lead to a cooling over the Northern Hemisphere, which eventually triggered the YD due to the existence of "sweet spot" of millennial-scale variability during glacial periods and positive feedbacks. I do see the potential of this mechanism, which enables an improvement of our understanding of YD dynamics.

Response #2: Again, we thank the reviewer for these positive comments and for summarising well what is essentially a very simple idea. We agree that there is clear potential for the LSE to have triggered the YD, and we very strongly feel that the hypothesis merits further consideration.

Comment #3: However, I'm a bit suspicious of its reliability. Some points are summarized in the following: 1) Responses of ocean circulation (AMOC) to a Northern Hemisphere volcano eruption is not that supportive of authors' argument. According to Pausata et al 2015 (PNAS), eruption's effect on AMOC is positive (strengthening) rather negative (weakening) at the first 20 years after the eruption, contrast to the weakening AMOC during YD.

Response #3: As we noted in our preliminary response online, the reviewer is correct that Pausata et al (2015) found that an eruption would strengthen AMOC. Other modelling studies based on historical data also suggest that eruptions may strengthen AMOC (Ottera et al., 2010; Swingedouw et al., 2014; Ding et al., 2014). However, other models suggest that AMOC may intensify initially, but then weaken after about a decade (Mignot et al., 2011). A modelling study by Schleussner and Feulner (2013) suggested that volcanic eruptions occurring during the last millennium triggered increased Nordic Sea sea ice extent which weakened AMOC and eventually cooled the entire North Atlantic Basin. Other research finds that North Atlantic sea ice growth following a negative forcing weakened oceanic convection and northward heat export during the Little Ice Age (Lehner et al., 2013). These are all studies focussing on eruptions that occurred over the last 1000 years, and they still yielded contradictory results. Therefore, we feel that how an eruption might affect AMOC at ~12.9 ka BP is still essentially unknown.

We thank the reviewer for raising this point, and we have now included the following text in an enhanced discussion to address it:

‘Although the radiative effects associated with volcanic aerosols are reasonably well understood, the systematics of how volcanic eruptions affect atmospheric and oceanic circulation are less well constrained. Research suggests that volcanic eruptions affect a wide variety of atmospheric phenomenon, but the exact nature of these links remains unclear. For example, Pausata et al. (2015) used a climate model to conclude that high latitude NH eruptions trigger an El Niño event within 8-9 months by inducing a hemispheric temperature asymmetry leading to southward Intertropical Convergence Zone (ITCZ) migration and a restructuring of equatorial winds. The model also suggests that these eruptions could lead to AMOC shifts after several decades, consisting of an initial 25-year strengthening followed by a 35-year weakening, illustrating the potential for climate effects extending well beyond sulphate aerosol atmospheric residence times. Several modelling studies based on historical data suggest that eruptions may strengthen AMOC (Ottera et al., 2010; Swingedouw et al., 2014; Ding et al., 2014) but also increase North Atlantic sea ice extent for decades to centuries following the eruption due to the albedo feedback and reductions in surface heat loss (Ding et al., 2014; Swingedouw et al., 2014). Other models suggest that AMOC may intensify initially, but then weaken after about a decade (Mignot et al., 2011). A modelling study by Schleussner and Feulner (2013) suggested that volcanic eruptions occurring during the last millennium triggered increased Nordic Sea sea ice extent which weakened AMOC and eventually cooled the entire North Atlantic Basin. Importantly, Schleussner and Feulner (2013) concluded that short-lived volcanic aerosol forcings triggered “a cascade of sea ice-ocean feedbacks in the North Atlantic, ultimately leading to a persistent regime shift in the ocean circulation”. Other research finds that North Atlantic sea ice growth following a negative forcing weakened oceanic convection and northward heat export during the Little Ice Age (Lehner et al., 2013). Quantifying the long-term influences of single volcanic eruptions is confounded by the effects of subsequent eruptions and other factors (e.g., solar variability, El Niño events), which can overprint more subtle feedbacks. For example, model results looking at recent eruptions found evidence that different types of eruptions can either constructively or destructively interfere with AMOC strength (Swingedouw et al., 2014). Therefore, despite increasingly clear indications that volcanic eruptions have considerable long-term consequences for atmospheric and oceanic circulation, the full scale of these shifts is currently not well understood even over the last two millennia, and are essentially unknown under Glacial boundary conditions.’

Comment #4: 2) Effect of southward ITCZ shift will lead to an increase of salinity in the North Atlantic subtropics, which will also act as a negative feedback to a potential weakening AMOC (Schmidt et al 2006 Nature).

Response #4: We have now included the reference suggested along with a new paragraph of text:

‘MWP-1B may have cooled the SH and strengthened AMOC, prompting northward migration of the ITCZ and NH mid-latitude westerlies to achieve equilibrium with high insolation conditions, thereby rapidly reducing sea ice

extent and warming Greenland, but this requires further research, particularly because the source, duration, and timing of MWP-1B are still unclear. Reduced oceanic salt export within the North Atlantic subtropical gyre, as is characteristic of stadials, may have preconditioned the North Atlantic toward vigorous AMOC following the initial migration of atmospheric circulation back to the north (Schmidt et al., 2006).'

Comment #5: 3) Although the ice volume during YD is beneficial to the occurrence of millennial-scale variability (Zhang et al 2014 Nature), the high CO2 level (250 ppm) will shift the "sweet spot" to a lower level of global ice volume (Zhang et al 2017 Nature Geo). This will weaken the arguments proposed by the authors.

Response #5: We thank the reviewer for raising this potential complicating factor, and we now discuss this point:

'The apparent high sensitivity of the climate system to millennial-scale climate change during times of intermediate ice volume is well documented (e.g., Zhang et al., 2014; Zhang et al., 2017), and here we investigate this further by examining the timing of Greenland Stadials relative to ice volume (estimated using Red Sea sea level (Siddall et al., 2003)). The timings of 55 stadial initiations as compiled in the INTIMATE (INTegration of Ice core, MArine, and TErrestrial) initiative (Rasmussen et al., 2014) are compared relative to ice volume, and indeed a strong bias towards intermediate ice volume conditions exists, with 73% of the millennial scale cooling events occurring during only 40% of the range of sea level across the interval from 0-120 ka BP (Figure 6). The distribution of events suggests that the most sensitive conditions are linked to ice volume associated with a sea level of -68.30 m below modern sea level. This intermediate ice volume was commonplace from 35-60 ka BP, and particularly from 50-60 ka BP. However, over the interval from 0-35 ka BP, these ideal intermediate ice volume conditions only existed during a short interval from 11.8-13.7 ka BP, and optimal conditions were centred at 13.0 ka BP (Figure 6). These results are broadly consistent with previous research (e.g., Zhang et al., 2014; Zhang et al., 2017), but the timing and duration of the most sensitive interval of time, and the likelihood that a forcing produces a longer-term cooling event, may ultimately depend on a more complex interplay between ice volume and atmospheric CO2 (Zhang et al., 2017). Atmospheric pCO2 during the YD initiation was relatively high (~240 ppmv) and could therefore affect the timing of ideal conditions for abrupt climate change in conjunction with ice volume, but their precise interdependence is still unclear. Finally, a frequency distribution of the sea level change rate associated with each stadial indicates that whatever mechanism is responsible for triggering a stadial operates irrespective of whether sea level (i.e., ice volume) is increasing or decreasing (Figure 6b). Because active ice sheet growth should discourage meltwater pulses, this observation seemingly argues against meltwater pulses as the sole trigger for initiating stadials.'

Comment #6: Nevertheless, I do see a potential of LSE (or northern hemisphere volcanic eruption) as a trigger to YD

Response #6: We agree, and thank the reviewer for this supportive comment. We see no reason why a well-dated, very high-sulphur, and high-latitude eruption should not at least be considered as a trigger for a cold event.

Comment #7: Muschitiello et al (2017 Nature Comm, also cited by the authors) recently proposed that the volcanic eruptions can effectively influence the mass balance of ice sheet via altering its surface albedo. This will promote the ice-sheet melting, leading to freshwater input to the North Atlantic and weakening the AMOC. I'm not an expert on data and climate response to the volcanic eruptions.

Response #7: This is a good point, but our understanding of the mechanisms invoked by that paper are that the eruptions have to be fairly proximal to the ice sheet to cause melting, i.e., long-lasting Icelandic eruptions affected the Fennoscandian Ice Sheet. It is therefore unlikely that the LSE affected the albedo of any major ice sheet.

Comment #8: But I think if the author can well improve the robustness of their arguments (probably by rephrasing the mechanisms), this will be a nice manuscript for Clim Past.

Response #8: We have now rephrased and extended our text considerably, and we think that the arguments are indeed much stronger. We also include a substantially revised section on relevant published climate modelling results that also discuss possible mechanisms. We thank the reviewer for highlighting that this rephrasing might be necessary, and we believe that this has indeed helped.

Comment #9: Line 145: Citation "Pasauta et al 2015 Tellus B" is not proper here. It should be Pausata et al 2015 PNAS.

Response #9: This has been corrected.

Reviewer #4 Comments and Responses:

Comment #1: The subject of the Younger Dryas cooling is one of considerable interest and fascination in the scientific community. Here, most research has been dominated by one theme that the cooling was triggered by a freshwater flood, or rerouting of meltwater, to the North Atlantic ocean. The idea that the YD cooling might have been triggered by a volcanic eruption has received much less attention and is very interesting.

Response #1: As the reviewer notes, there has been essentially no modern research on whether or not the Laacher See eruption could have triggered the YD, largely because the evidence available suggested that the LSE predated the onset of YD cooling (GS-1) by ~200 years. Given that the last few published review papers on the YD do not even mention volcanism, we feel that this is a valuable (and novel) contribution, and we are glad that the reviewer finds it interesting.

Comment #2: Overall, I really enjoyed the paper. It's very well written, easy to follow, and provides a nice break from the more typical meltwater-trigger hypothesis. Indeed, I found the discussion about the sensitive of climate to intermediate ice volume conditions, and the alignment of this 'ideal' configuration, to the timing of the YD very enlightening.

Response #2: We thank the reviewer for these supportive comments.

Comment #3: But whether a volcano actually triggered the YD is hard to tell from this paper. Yes, there was an eruption around the time of the YD cooling, but did it really produce a 1000-yr cooling? As such, the manuscript would have been vastly improved if the authors had done their own climate modeling. I think it would have been fantastic to try and see whether a volcano could have triggered a YD-like cooling. Indeed, the authors note that previous studies (fig 2) released 10-time LESS SO2 to the atmosphere than what is estimated here. Whether these experiments should be undertaken, I will leave that up to the authors, but I'm not going to rejecting this paper simply because they were not carried out.

Response #3: We really appreciate the reviewer's perspective on the inclusion (or not) of modelling. Although we agree that modelling is important, we feel that this is outside the scope of the current submission, and best left for future research. The reason is outlined at length in our response to Reviewer #2's comment #2, as well as in new text that we have added to discuss relevant models. Essentially, model simulations of the response of AMOC to volcanic forcing over the last 1000 years have yielded ambiguous results, with some models predicting that eruptions strengthen AMOC, and others predicting a weaker AMOC. Still others suggest initial strengthening, followed by long-term weakening. We strongly feel that under the considerably less-well constrained deglacial conditions, modelling results would be even more ambiguous, and results would not necessarily be robust. We hope that this manuscript would provide the motivation for substantial future modelling work on the triggering of the YD by the LSE, and we feel that modelling support (or not) would need to come from multiple climate studies conducted over several years.

Comment #4: Finally, I wasn't sure if the MWP-1b discussion was really needed. The existence of this period of rapid sea level rise is still very much debated, as is its source, with various camps arguing back-and-forth over an Antarctic or Laurentide contribution.

Response #4: We have toned down the MWP-1b discussion, and included more text, to emphasise that the provenance of the meltwater, and even its existence, is still uncertain and controversial.

Comment #5: Anyway, my overall opinion is that this is a very interesting paper and it should be published with minor corrections/edits.

Response #5: We thank the reviewer for this opinion, and for their comments. We have taken on board their suggestions, as well as those from the other reviewers, and we feel that our revised manuscript is substantially improved from the last submission.

<u>Responses to Short Comment by David Pyle</u>

Comment #1: For me the leading questions (which come out of this paper) are:
- was the LST eruption the source of a sulphur 'spike' in Greenland ice cores; and how could we test this assertion?

Response #1: This could be tested by searching for, and fingerprinting, ash in the ice containing the sulphur spike. We now also mention using triple sulphur isotopes to detect stratospheric aerosols as future work. Although this would not pinpoint the LSE as the cause, it would suggest that it was a large eruption where aerosols reached the stratosphere, reducing the likelihood of a localised Icelandic eruption being the origin of the sulphur spike. We now state this as critical future work.

Comment #2: What was the total volatile yield from the LST eruption; and what new measurements are needed to improve on this assessment. (And did the halogen release have any impact?)

Response #2: The magma that fed the LSE was undoubtedly sulphur-rich (see Textor et al., 2003; and Table 5 in Scaillet et al. 2003), and would have injected significant amounts of H_2S and/or SO_2 into the atmosphere, enabling it to have an effect on climate comparable to eruptions that are substantially larger in total erupted volume. Estimating volatile yields from ancient eruptions is difficult and complex (see the review in Scaillet et al., 2003), but we have updated this section (Section 2.0, Background) to include work that builds on the work of Schmincke et al., (1999) that was previously cited. A full petrological study of sulphur in the magma, using updated methodology (for example, that of Vidal et al., (2016)) is beyond the scope of this paper, but we hope that our work will stimulate others to undertake this important study. We have now included the most recent estimates of total sulphur produced by the eruption, as reported in the suggested papers. We have also calculated a new estimate based on the difference between the petrologic and actual sulphate yields of 17 explosive eruptions (mostly from Shinohara 2008, *Reviews in Geophysics*), which is consistent with the range reported by Textor et al., 2003. We also now mention that halogen release could also play a role.

Comment #3: What cascade of physical processes could lead to the observed pattern of response seen for the onset of the YD; and is a volcanic eruption a sufficient driver, on its own.

Response #3: The mechanisms that lead from the initial hemispheric cooling from the LSE sulphate cloud (itself uncontroversial) through to the YD as experienced across western Europe, is described in Section 3.3. Crucially, the LSE occurred during a period of intermediate ice cover, when we infer that the climate was particularly sensitive to short-term cooling events (as has been concluded previously). Thus, we consider that the sulphur-rich LSE was capable of acting as the catalyst for longer term cooling and climatic reorganisation characteristic of the YD. We have also included a much more detailed discussion regarding the positive feedback, and outlining in detail the substantial amount of recent research that has similarly concluded that volcanic eruptions can have long-term consequences for climate.

Detailed points

Line 44 – there is also a documented enrichment in noble metals (e.g. platinum), at this stratigraphic level both in North America and Europe (Moore et al., Scientific Reports 7 Article Number: 44031, 2017; and papers by A Andronikov).

Response: We have now included references to the paper by Moore et al. and Andronikov. We note that in the Supplemental Material of Moore, the authors state that the LSE eruption occurred 200 years before the YD – which appears to reflect the consensus perspective, which unfortunately is also incorrect, as we discuss in the manuscript. In a more recent publication (Wolbach et al., 2018, J. of Geology), the eruption is also dismissed as being the source of the Pt spike, with the authors stating that the eruptions occurred 200 years too early. In their Figure 7 (and elsewhere), the timing of the spike is nearly indistinguishable from the timing of the eruption. We feel that there is a very reasonable possibility that the Pt spike is not derived from a cometary airburst, but instead is derived from the LSE. We now include text alluding to this possibility, but would like to reserve an in-depth discussion for after we are able to sample the LST for Pt concentrations directly.

Line 50 – 'new support' yes, but not much new evidence.

Response: We explain how short-term, volcanogenic sulphate-induced hemispheric cooling could have acted as the trigger for the longer-term cooling and climatic changes associated with the YD. In the last few comprehensive reviews of the Younger Dryas event (e.g., Carlson 2010; Fiedel 2011) there was no mention of the LSE or any volcanic eruption as a trigger. We therefore feel that we use existing evidence to arrive at a very novel, provocative, and important conclusion. We also identify a candidate sulphur spike within the GISP2 ice core, and provide a new estimate for the total sulphate emitted by the eruption.

Line 50 – ages: some explanation is needed about the framing of time in the paper, as the model ages are derived from multiple approaches.

Response: We have now clarified the origin of the dates, as well as added dating uncertainties where needed.

Lines 53- 56: the emphasis here isn't quite right. The evidence for a 200-year time break between LST and the onset of 'YD' conditions in continental Europe remains firm. What has changed – since Lane et al., 2015, is the recognition that the onset of YD is time-transgressive. So – by inference – LST overlaps with GS-1 and the onset of YD as recorded in ice chemistry in Greenland.

Response: There is a lot of grey area here, but we believe that it is correct as we have it written. The YD is a term that has its origins in central European climate studies that detected the change in atmospheric circulation. So whereas GS-1 cooling did lead to the manifestation of the YD in central Europe, GS-1 cooling was not the YD itself.

Line 57 – does the 'GICC05modelext chronology' need a word or two of explanation
Response: We have now clarified this at the first occurrence in the text.

<u>LST Impacts</u>
Line 74: see also the extensive work by Felix Riede on the impacts of the LST:

Book, 2017 - Splendid Isolation : The eruption of the Laacher See volcano and southern Scandinavian Late Glacial hunter-gatherers. / Riede, Felix. Aarhus : Aarhus Universitetsforlag, 2017. 214 p.

2016 - Changes in mid- and far-field human landscape use following the Laacher See eruption (c. 13,000 years BP). / Riede, Felix. In: Quaternary International, Vol. 394, 02.2016, p. 37-50.

2012 - Bayesian radiocarbon models for the cultural transition during the Allerod in southern Scandinavia, Riede, Felix; Edinborough, Kevan, JOURNAL OF ARCHAEOLOGICAL SCIENCE 39, 744-756

2008 - The Laacher See-eruption (12,920 BP) and material culture change at the end of the Allerod in northern Europe, Riede, Felix, JOURNAL OF ARCHAEOLOGICAL SCIENCE 35, 591-599

Response: We thank Professor Pyle for bringing these studies to our attention and we now reference Riede (2008) and Riede (2016) in the appropriate section.

<u>Volcanic Emissions</u>
Lines 78 - 85 – this section needs some critical revision and updating. Harms and Schmincke (2000) estimated, using mass balance, an SO2 yield of 20 Tg. Harms et al. (2004) did some experiments on LST magmas and determined the P, T , H2O conditions under which the magma was stored; you could revisit the calculations of

Harms & Schmincke to re-estimate the S and water budgets of the system – taking account of the work that Bruno Scaillet and colleagues have done on other systems. The '150 Mt' value should be cited as Schmincke et al (1999, Quaternary International, 61, 61-72) – it is, as the authors say 'highly speculative' and based on using the 'Pinatubo multiplier'; this can certainly be improved upon, rather than being taken as a starting point for the argument. Similar calculations have been attempted by Textor et al., 2003, (Geol Soc London Spec Pub, 'Volcanic Degassing', 213, 307-328); who also estimated the total halogen yield. Discussion on volcanic emissions Recent papers may also add a little to the discussion here: for example - - Colose, C.M., A.N. LeGrande, and M. Vuille, 2016: Hemispherically asymmetric volcanic forcing of tropical hydroclimate during the last millennium. Earth Syst. Dyn., 7, 681-696, doi:10.5194/esd-7-681-2016. - LeGrande, A.N., K. Tsigaridis, and S.E. Bauer, 2016: Role of atmospheric chemistry in the climate impacts of stratospheric volcanic injections. Nature Geosci., 9, no. 9, 652-655, doi:10.1038/ngeo2771.

Response: These are very good points and we have extensively updated this section. We now include the improved sulphur yield estimates of Textor et al., (2003), and note that these are still significantly greater than those emitted by the climatically-important Pinatubo eruption. A detailed petrological investigation of S contents in LS tephra would be a very interesting, substantial study, and we hope our research will encourage other researchers to look into this. As mentioned above, we have derived a new estimate for total sulphate released by using the mean of a range of petrologic to actual ratios reported in Shinohara 2008.

Line 178 – 'five years' may be an overestimate: in Graf and Timmreck's model, sulphate aerosol had an e-folding time of 11 months; and the detectable signal of volcanic stratospheric sulphate aerosol is usually considered to be less than three years.

Response: The text has been modified throughout.

Lines
180 – 195 – there's not really any new evidence here?

Response: Here we are drawing links between different a wide range of studies to establish a plausible mechanism to trigger long-term cooling following a short-term volcanic forcing of climate. We have added substantially more discussion to make this point clearer.

Line 208 – the magnitude of the eruption is not relevant, it's the magnitude of the gas release that is the key point. The LST magma is an unusual composition, so surely this is the starting point for why it may have had an exceptional impact?

Response: This is a very useful comment, and although we did appreciate that this was the case before, the manuscript did focus too much on magnitude and not enough on sulphur yield and the unusual composition of the magma. We have added a statement on the importance of the sulphur content of the erupting magma, as well as emphasising the sulphur yield rather than the magnitude throughout. We have also added a new figure, which clearly shows that the LSE was anomalous in terms of total sulphur emitted.

Lines 274 – 284: there still is no way of linking a sulphate peak in an ice core to a particular eruption, in the absence of any tephra so this remains speculative. It remains possible that sulphur mass-independent isotopic fractionation signals may help to identify plumes that entered the stratosphere (e.g. Martin et al., 2014, Volcanic sulfate aerosol formation in the troposphere, JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES Volume: 119 Issue: 22 Pages: 12660-12673), but this still won't help with source identification.

Response: This is an important point and we now discuss this in more depth, with the following text added to the conclusions:

"Similarly, volcanic sulphate 'triple' isotope ratios of sulphur and oxygen provide information regarding the residence time of volcanic plumes in the stratosphere. The majority of atmospheric processes encourage mass dependent fractionation; however rare mass-independent fractionation processes produce isotope ratios that do not behave according to predictions based on mass dependent processes (Martin et al., 2014). Historical volcanic eruptions where sulphate aerosols reached the stratosphere have been successfully identified in ice cores (Baroni et al., 2008; Savarino et al., 2003), indicating that the technique is effective at distinguishing large explosive eruptions from smaller local ones. This technique could also determine if the sulphate in the potential LSE sulphate spike reached the stratosphere. Although this would not necessarily confirm the LSE as the source, it would strongly suggest that the sulphate was sourced from a climatologically important eruption rather than a smaller Icelandic one."

Responses to Short Comment by Evzen Stuchlik

First, we would like to simply note that the goals of our submitted manuscript are straightforward: i) to highlight that the timing of the Laacher See eruption is indistinguishable from the initiation of cooling associated with the Younger Dryas, ii) to highlight the possibility that the effects of volcanic eruption can persist longer than just 1-3 years, and finally iii) that consequently the eruption should be viewed as a viable trigger for the Younger Dryas Event. In other words, if the LSE occurred at the correct time (and it appears that it did), and if an eruption of this scale and sulphur content could catalyse extended cooling (and it appears that it could), then logically the LSE should be considered a viable trigger for the Younger Dryas. Clearly more research needs to be conducted on this topic, but getting the idea out there is the key first step. We note that this is the only hypothesis where it is universally agreed that the proposed trigger actually occurred, and where it currently seems that the age coincides with the initiation of Younger Dryas cooling.

We have now added a section discussing the pros and cons of other proposed triggers. However, there are reams of papers discussing the pros and cons of the Younger Dryas Impact Hypothesis specifically, and providing a thorough review of all the evidence for or against this hypothesis is not possible or necessary. For example, it is true that the Laacher See Eruption would not account for the observed megafaunal extinctions across North America. However, recent papers [*Cooper et al.*, 2015; *Metcalf et al.*, 2016; *Rule et al.*, 2012; *van der Kaars et al.*, 2017] make an extremely strong case that this was caused by human migration (how else to explain the observation that the extinctions did not occur at the same time, and tracked human migration?), and that therefore the LSE (or an impact, or a meltwater pulse) would not have needed to cause any extinction. This perspective is also supported by the presence of other Younger Dryas-type events that apparently occurred during other Glacial terminations, e.g. TIII [*Broecker et al.*, 2010] but that were not associated with megafaunal extinctions, implying that neither YD-type climate change nor a bolide impact were the cause of the megafaunal extinctions. Furthermore, we do not argue that a bolide impact did not happen near the YD boundary (it may have), so defending the presence or absence of a Pt spike, shocked quartz, black carbon, nanodiamonds, etc. is well beyond the scope of the manuscript. That being said, we have added considerable more detail in the revised manuscript.

In response to your comments that are specific to our hypothesis:

1) Comment: Our 'statement indicating that Laacher see eruption (LSE) effect could last for some 5 years is in the contrast to surprisingly main conclusion not completely supported by own data and highly speculative that this event could trigger YD cooling.'
1) Response: We discuss a proposed ice/ocean feedback in detail in Sections 3.2, 3.3., and 3.4, and the concept of a positive feedback amplifying the original volcanic forcing is increasingly commonplace (see recent paper by Kobashi et al., Scientific Reports 2017 for example). There are now several papers that suggest the presence of a sea ice/oceanic circulation feedback that amplifies the initial short-lived aerosol cooling, and we will discuss these further in the revised manuscript as suggested by another

reviewer. We therefore feel that the concept of a longer-term volcanic forcing is well-defended already by several pages of text as well as previously published papers (these will be included and discussed in the revisions); we do not feel that it is highly speculative if you are familiar with this most recent literature. Upon any revision, we will revise this text to ensure that this message is clear, and describe the positive feedback mechanism in more detail.

2) Comment: ‘Resulting the title of msc starting with “Reevaluation” is inappropriate to the msc content.’

2) Response: The Laacher See eruption was very briefly mentioned as a proposed trigger for the YDE, before it was discarded. However, the most recent lake core and ice core data suggest that the YD cooling occurred synchronously with the LSE, so we feel that ‘Re-evaluating’ is the correct word to use here. We were not the first to suggest the eruption as a trigger, although we are ‘re-evaluating’ the eruption’s climatological consequences in a modern context. Still, another reviewer raises this same issue, so although we feel that this is in fact the correct term, we clarify why we chose to use this term in the title.

Finally, we thank you for the papers that you have provided. We will include these in any revisions, where relevant and appropriate.

References:

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van der Kaars, S., G. H. Miller, C. S. M. Turney, E. J. Cook, D. Nurnberg, J. Schonfeld, A. P. Kershaw, and S. J. Lehman (2017), Humans rather than climate the primary cause of Pleistocene megafaunal extinction in Australia, *Nat Commun*, *8*, 7.

Re-evaluating the link between the sulphur-rich Laacher See volcanic eruption and the Younger Dryas cold event

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Abstract. The Younger Dryas is the most well-documented millennial-scale cooling event of the Quaternary, but the mechanisms responsible for its initiation remain elusive. Here we use a recently revised chronology for the GISP2 ice core ion dataset from the Greenland ice sheet to identify a large volcanic sulphur spike coincident with both the unusually sulphur-rich Laacher See volcanic eruption and the onset of Younger Dryas-related cooling in Greenland (GS-1)(i.e., the most recent abrupt Greenland millennial-scale cooling event, Greenland Stadial-1; 'GS-1') in Greenland. Lake sediment and stalagmite records confirm that the eruption's timing was indistinguishable from the onset of cooling across the North Atlantic, but that it preceded westerly wind repositioning over central Europe by ~200 years. We suggest that the initial short-lived volcanic sulphate aerosol cooling was amplified by oceanic circulation shifts and/or sea ice expansion, gradually cooling the North Atlantic region and incrementally shifting the mid-latitude westerlies to the south. The aerosol-related cooling probably only lasted 1-32-4 years, and the majority of Younger Dryas-related cooling was instead due to the sea ice-ocean circulation this positive feedback, which was particularly effective during the intermediate ice volume conditions characteristic of ~13 ka BP. We conclude that the large and sulphur-rich Laacher See eruption should be considered a viable trigger for the Younger Dryas.

1 Introduction

The Younger Dryas (YD) represents the archetypal millennial-scale climate shift. The YD occurred during the last deglaciation and is often described as a brief return to near-glacial conditions in northern Europe. Research now indicates that the YD was indeed characterised by cold conditions across the North Atlantic and Europe (Carlson et al., 2007; von Grafenstein et al., 1999), but also by a southward-shifted westerly wind belt over Europe (Bakke et al., 2009; Brauer et al., 2008; Lane et al., 2013a; Baldini et al., 2015b), a southwardly-shifted Intertropical Convergence Zone (Shakun et al., 2007; Chiang and Bitz, 2005), increased moisture across the southwest of North America (Polyak et al., 2004; Asmerom et al., 2010), and potential warming in parts of the Southern Hemisphere (Bereiter et al., 2018; Kaplan et al., 2010).

The most widely accepted explanation for the YD involves meltwater forcing-induced weakening of Atlantic Meridional Overturning Circulation (AMOC) (Berger, 1990; Alley, 2000; Broecker et al., 2010; Johnson and Mcclure, 1976). Broecker 1989 Nature. Johnson 'and McClure [1976] which calls upon diversions of meltwater between the Mississippi and St. Lawrence. Rooth [1982] pursued this idea by suggesting that the impact of this diversion was to turn off deepwater production

~~in the North Atlantic.~~ ~~The freshwater, generally proposed to have originated from North American proglacial lakes, is theorised to have formed a freshwater cap over the North Atlantic, encouraging sea ice expansion, prohibiting the formation of North Atlantic Deep Water, and consequently weakening (or shutting down) AMOC.~~ Initial support for this theory included elevated δ^{18}O values in Gulf of Mexico sediment dating from the early YD (implying that meltwater was rerouted elsewhere) (Broecker et al., 1988; Flower and Kennett, 1990; Teller, 1990). T~~However, t~~he meltwater was originally proposed to have travelled to the North Atlantic via the St Lawrence Valley, but results~~exhaustive searches have~~ have so far revealed only limited geological evidence for a massive flux of freshwater coincident with the YD initiation (Broecker, 2006b; Rayburn et al., 2011).~~.~~ ~~–~~ The freshwater pulse may have followed another route to the ocean, and ~~more recent~~other research has proposed the Mackenzie Valley~~-~~ (Condron and Winsor, 2012; Murton et al., 2010) ~~(Muschitiello et al., 2015)~~as a possible ~~an~~ ~~an~~ alternative, and the Fennoscandian Ice Sheet as an alternate source (Muschitiello et al., 2015) ~~(Condron and Winsor, 2012; Murton et al., 2010) (Condron and Winsor, 2012; Murton et al., 2010)~~. However, very recent surface exposure ages suggest that the route to the North Atlantic from Lake Agassiz was indeed free of ice before the YD initiation (Leydet et al., 2018)~~ et al.,~~, coinciding with evidence for freshening of the Gulf of St Lawrence (Levac et al., 2015), illustrating that meltwater could indeed have followed the St Lawrence Valley route to the North Atlantic. However, the issue concerning the ~~uncertain~~ routing of a meltwater pulse illustrates a key uncertainty inherent to the meltwater pulse hypothesis. Another complication is that efforts to model the AMOC response to freshwater inputs under deglacial boundary conditions have yielded equivocal results (e.g., Meissner, 2007). Consequently, although meltwater forcing remains the most widely researched cause of the YD, it is not universally accepted. Broad agreement does exist that AMOC weakening was associated with the YD onset (Lynch-Stieglitz, 2017), but the driver of this weakening remains unclear.

Other recent research has proposed that a large impact event, or events, over North America may have triggered the YD (Firestone et al., 2007; Kennett et al., 2009). The Younger Dryas Impact Hypothesis (YDIH) is supported by the discovery of iridium, shocked quartz, platinum, and millions of tons of impact spherules at the YD boundary layer (Kennett et al., 2009; Wittke et al., 2013; Wu et al., 2013). Wolbach et al. (2018a) use charcoal and soot evidence from 152 sedimentary sequences and elevated concentrations of platinum at 26 sites (including Greenland) to argue that a cometary impact at the beginning of the YD triggered the largest wildfires of the Quaternary, consuming 9% of global terrestrial biomass. The YDIH has proven remarkably ~~controversial,~~controversial, and different researchers suggest either terrestrial origins for the same evidence, that the evidence is not unique to the YD boundary, or that the YD boundary layer was misidentified ~~e.g.,~~ (e.g., Pinter et al., 2011; van Hoesel et al., 2015; Holliday, 2015; Scott et al., 2017; Daulton et al., 2017).

Here we summarise but do not argue extensively for or against any of these established hypotheses. Instead, we ~~re-re-introduce and provide new support for~~~~evaluate~~investigate the hypothesis that the YD was triggered by the ~12.9 ka BP eruption of the Laacher See volcano, located in the East Eifel Volcanic Field (Germany). Earli~~ery~~ research briefly ~~briefly considered~~ alluded to the ~~mentioned the~~ eruption as a possible causative mechanism for the YD (e.g., Berger, 1990; Bogaard et al., 1989).

However, because the meltwater pulse hypothesis was already popularised, and because the effects of volcanic eruptions on climate would escape detailed quantification until after the 1991 Pinatubo eruption, ~~the~~the ~12.9 ka BP Laacher See eruption as ~~the~~a YD trigger ~~failed~~never ~~to~~ gained traction. ~~However~~Importantly, the concept was effectively dismissed ~~because~~ after lacustrine evidence across central Europe appeared to indicate that the YD's clearest expression appeared ~200 years after the Laacher See Tephra within the same sediments (e.g., Brauer et al., 2008; Brauer et al., 1999a; Hajdas et al., 1995). For example, Schmincke et al. (1999) state that "The Younger Dryas cooling period clearly was not triggered by LSE as formerly thought because it started ca. 180 years after the eruption~~.~~", reflecting the accepted sequence of events at that time. However, the identification of the Vedde Ash chronostratigraphic unit within Meerfelder Maar (Germany) lake sediments has permitted improved correlation with Greenland ice core records, which also contain the same ash (Lane et al., 2013a)~~{Lane, 2015 #2669}~~ ~~(LANE)~~. This revised chronological framework ~~recently revised stratigraphic frameworks for key climate archives~~ now strongly suggests ~~suggest~~ that the 12.880 ka BP Laacher See eruption was in fact synchronous with cooling associated with the YD onset (i.e., the most recent abrupt Greenland millennial-scale cooling event, Greenland Stadial-1; 'GS-1'), but preceded major atmospheric circulation shifts over central Europe (Rach et al., 2014)~~(RACH)~~.

Furthermore, w ~~WHere w~~e ~~also~~ utilise ~~use~~ ion data from the Greenland ice core GISP2 ~~ion data~~ (Zielinski et al., 1997) on the most recent chronological model for the core (the GICC05modelext chronology (Seierstad et al., 2014)) to identify a large volcanogenic ~~sulphur~~ sulphate spike whose timing ~~is consistent with~~coincides with both ~~radiometric ages for~~ the Laacher See eruption and ~~its the timing Laacher See eruptionas recorded in varved lake cores~~the initiation of GS-1 related cooling. ~~, and~~ We argue that the initial, short-lived volcanogenic aerosol cooling triggered a sea-ice/AMOC positive feedback that ~~led~~ caused both basin-wide cooling and the~~to~~ dynamical climate shifts ~~changes~~ most closely associated with the YD.~~.; t~~ Therefore ~~Therefore, although early research suggested the Laacher See as a causative mechanism for the YD, and subsequent research dismissed the early version of the hypothesis,~~ we are 're-evaluating' the Laacher See eruption's role in triggering the YD, building on ~~early~~ research that ~~first~~initially briefly ~~suggested~~mentioned the eruption as a causative mechanism for the YD, and later research that dismissed the original version of the concept.

2 Background

Laacher See volcano, Germany, is situated in the East Eifel Volcanic Field, which is part of the West European rift system (Baales et al., 2002; de Klerk et al., 2008) (Figure 1). The Laacher See Eruption (LSE) occurred at ~12.880 ± 0.040 ka BP based on the position of tephra within regional varved lake sequences (e.g., Wulf et al., 2013; Brauer et al., 1999a; Lane et al., 2015; Bronk Ramsey et al., 2015), consistent with radiocarbon (12.934 ± 0.165 cal ka BP (Baales et al., 2002)) and $^{40}Ar/^{39}Ar$ (12.9 ± 0.50~~0~~ ka BP (van den Bogaard, 1995)) ages for the eruption; the absolute age of the eruption is therefore well constrained. The eruption was one of the largest in Europe during the late Quaternary and dispersed over 20 km^3 of pumice and ash over >230,000 km^2 of central Europe and beyond (Baales et al., 2002; Bogaard and Schmincke, 1985). The eruption consisted of alternating Plinian and phreatomagmatic phases; Plinian columns exceeded 20 km height and injected ash and volcanic gas into the stratosphere (Harms and Schmincke, 2000). Direct effects of the LSE included ash deposition, acid rain, wildfires, and increased precipitation, all of which could have affected the local and far-field ecology and cultures at the time (de Klerk et al., 2008; Baales et al., 2002; Engels et al., 2016; Engels et al., 2015).

The LSE discharged up to 6.3 km^3 of unusually sulphate-rich, evolved phonolite and sulphide-rich mafic phonolite magma from a strongly compositionally zoned reservoir (Harms and Schmincke, 2000). Estimating SO_2 release from eruptions ~~recorded~~preserved in the geologic record is difficult and relies on petrologic comparisons between the sulphur content of melt inclusions (the magma at depth) and the sulphur content of glass in erupted products (the degassed magma) (Devine et al., 1984; Scaillet et al., 2004; Textor et al., 2003; Vidal et al., 2016)~~(e.g., Devine et al., 1984;(Devine et al., 1984) (Scaillet et al., 2004)see Scaillet et al., 2003, and references therein;(Textor et al., 2003) Textor et al., 2003A;(Vidal et al., 2016) Vidal et al., 2016)~~. Petrologic methods can lead to underestimations of total sulphur release by up to ~~one to~~ two orders of magnitude relative to ~~other methods, e.g.,~~ satellite data (Gerlach et al., 1996; Scaillet et al., 2004) ~~(Gerlach et al., 1996; see Scaillet et al., 2003)~~. For example, s~~S~~atellite-derived estimates ~~of the SO_2 content~~ of the 1991 Pinatubo eruption's SO_2 content (15–20 megatonnes (Mt)), ~~-~~are considerably higher than the 0.11 ~~Mt~~Mt- petrologic estimate (Sheng et al., 2015), ~~-at 15–20 Mt (Sheng et al., 2015), a~~and similar discrepancies between petrologic and satellite-derived estimates of SO_2 emissions exist for other modern eruptions (Scaillet et al., 2004) (Figure 2). The consensus is that this excess sulphur is contained in a sulphur-rich vapour phase that is released prior to and during the eruption (Gerlach et al., 1996; Wallace, 2001). Petrologic studies by Harms and Schmincke (2000) ~~Petrologic studies~~ suggest that the LSE released 3.8 Mt of SO_2, but considering a sulphur-rich vapour phase they concluded that the eruption released at least ~ 20 megatons ~~(Mt)~~ of SO_2 ~~sulphur~~ into the stratosphere. ~~, while~~Textor et al. (2003)~~Textor et al. (2003A) concluded~~suggested that the eruption released between ~~3.38~~6.76 ~~Mt~~ and 104.8~~of sulphur (assuming pure SO_2) to 52.4~~ Mt of ~~sulphur~~SO_2 ~~(assuming pure H_2S)~~, when considering uncertainties regarding the oxidation state of the vapour phase. ~~–~~ Despite these ~~uncertainties~~unknowns, the amount of SO_2 released by the LSE was considerably higher than the petrologic estimates, and Schmincke et al. ~~,~~(1999) speculated a maximum release of 300 Mt[BJ1] SO_2, assuming the same relationship between petrologic ~~studies~~ and observed ~~studies~~values as for the Pinatubo eruption. We utilise a similar but more comprehensive ~~another~~ approach, taking the mean value of the relationship between petrologic and observed values of the 16 non-basaltic explosive eruptions catalogued by Shinohara (2008) plus estimated values for the large 1257 AD Samalas eruption (Vidal et al., 2016) ~~(REF caption)~~ to estimate that the LSE released ~83 Mt SO_2, although the range in possible values is substantial ~~and limits the accuracy of this estimate~~. ~~Despite the uncertainty, these values are (Harms and Schmincke, 2000), considerably more~~ higher ~~than petrologic estimates of the amount of~~ SO_2 ~~sulphur erupted during the 1991 Pinatubo eruption (0.11 Mt) (Schmincke et al., 1999).~~A variety of estimated values clearly exist, but the eruption ~~probably~~almost certainly released ~~at least as much~~more SO_2 ~~as~~than the Pinatubo eruption (~20 Mt), and possibly ~~nearly~~even ~~as~~more than the ~~much as~~ 1815 Tambora eruption (~70 Mt) (Figure 2). ~~However, petrologic methods typically severely underestimate the total sulphur~~

released to the atmosphere (often by a factor of 10–100) (Harms and Schmincke, 2000). For example, sSatellite-derived (TOMS) estimates of the SO_2 content of the 5 km^3 Pinatubo eruption are considerably higher than the 0.11 Mt petrologic estimate, at 15–20 Mt (Sheng et al., 2015), and similar discrepancies between petrographic petrologic and satellite-derived estimates of SO_2 sulphur emissions exist for other modern eruptions(Scaillet et al., 2004) (Scaillet et al., 2003). Explanations The consensus is that thisfor this excess sulphur sulphur include is the presence ofcontained in a sulphursulphur-rich vapour phase that is released prior to and during the eruption(Gerlach et al., 1996; Wallace, 2001) (Harms and Schmincke, 2000) (see Gerlach et al., 1996; Wallace, 2001). Consequently, the amount of the true SO_2 amounts sulphur contentreleased by of the LSE was probably considerably higher than the petrologic estimates: , with Baales et al. (2002)Schmincke et al., (1999)(1999) suggesting speculate a maximum sulphur SO_2 release of 150 Mt[BJ2], approximately nine times larger than that associated with the 1991 Pinatubo eruption. However, unlike the Pinatubo and Tambora eruptions, sulphate aerosols released produced by theduring the LSE were largely restricted to the Northern Hemisphere (NH) (Figure 32), leading to strong cooling in the stratosphere that affected the NH disproportionately (Graf and Timmreck, 2001). Scavenging of SO_2 by incorporation into ice particles may reduce the stratospheric load of erupted SO_2 by ~20 %(Textor et al., 2003a) (Textor et al., 2003B).

Both Pinatubo and the LSE were Magnitude 6 (M6) eruptions, where ‘magnitude’ is a measure of eruption size referring to the amount of tephra and lavamaterial erupted (Deligne et al., 2010) (Deligne et al., 2010 JGR) (on a logarithmic scale;). However, the cooling effects of a volcanic eruption isare controlled by the amount of sulphursulphur released, and not necessarily the eruption size (Rampino and Self, 1982). Unfortunately, Tthe amount of sulphur released by prehistoric eruptions is often ambiguous due to the fact that much of the sulphur exists in a volatile phase and is not retained in the rock record. In general, magnitude and erupted sulphur amounts are well correlated (Oppenheimer, 2003) (Oppenheimer, 2003), and therefore magnitude is often used as a surrogate for sulphur yield. All the available evidence suggests that if the LSE deviates from this trend, it was anomalously enriched in sulphursulphur relative to its magnitude (Baales et al., 2002; Scaillet et al., 2004) (Baales et al., 2002; Scaillet et al., 2003), and that it therefore should have produced significant NH cooling.

3 Results and Discussion

3.1 The timing of the Laacher See Eruption relative to the Younger Dryas

Key research using European lake sediment archives ~~suggested that~~containing the Laacher See Tephra (LST) suggested that the LSE preceded the YD onset by ~200 years (Hajdas et al., 1995; Brauer et al., 1999a). For example, the LST appears very clearly in the Meerfelder Maar sediment core (Germany), and it does indeed appear to predate the YD (e.g., Brauer et al., 1999a). However, the recent discovery of the Icelandic Vedde Ash in Meerfelder Maar sediments has revised the ~~stratigraphic framework~~relative timing of key climate archives around the North Atlantic (Lane et al., 2013a). It is now apparent that the clearest hydroclimatic expression of the YD in central Europe lags Greenland cooling associated with ~~Greenland Stadial 1 (~~GS-1~~)~~ by 170 years (Figure 4~~3~~). Specifically, several lake sediment cores from different latitudes have revealed that westerly wind position was time transgressive, and gradually shifted to the south following the ~~NGRIP ice core defined~~ initiation of GS-1 cooling (Bakke et al., 2009; Lane et al., 2013a; Brauer et al., 2008; Muschitiello and Wohlfarth, 2015).~~ An absolutely-dated, speleothem-based wind strength proxy record from northern Spain has independently confirmed this general southward migration of the westerlies (Baldini et al., 2015b).~~

Although the LSE ~~apparently~~ preceded the most clearly expressed dynamical climatic change associated with the YD in central Europe, its timing (12.880 ± 0.040 ka BP) ~~–~~is indistinguishable from the Greenland temperature decrease ~~associated with~~ leading into ~~the~~ GS-1, beginning at~~ onset~~ 12.870 ± 0.138 ka BP (Rach et al., 2014; Steffensen et al., 2008; Rasmussen et al., 2014) (GS-1 is defined as starting at 12.846 in the NGRIP record, but abrupt cooling predates this by ~24 years)~~-~~ (Figure 4~~3~~). In central Europe, hydrogen isotope ratios of land plant-derived lipid biomarkers from Lake Meerfelder Maar ~~indicate~~ confirm that North Atlantic atmospheric cooling began at ~12.880 ka BP (Rach et al., 2014), ~~preceding the atmospheric response associated with meridionally displaced westerly winds at the same site but~~ synchronous with both the onset of Greenland GS-1 related cooling~~ (Steffensen et al., 2008; Muschitiello et al., 2017)~~and the LSE, ~~(importantly, in MFM the first data point suggesting cooling occurs immediately above the LST (Rach et al., 2014)),~~ but preceding the atmospheric response associated with meridionally displaced westerly winds at the same site (importantly, in MFM the first data point suggesting cooling occurs immediately above the LST (Rach et al., 2014)).~~(Rach et al., 2014).~~ This observation is further supported by speleothem $\delta^{18}O$ records from La Garma and El Pindal caves in northern Spain (Baldini et al., 2015b; Moreno et al., 2010), ~~-~~Swiss lake sediment $\delta^{18}O$ records (Lotter et al., 1992), Lithuanian lake sediment trace element profiles (Andronikov et al., 2015), and Swiss lake organic molecule-based (BIT and TEX_{86})~~ based~~ temperature reconstructions (Blaga et al., 2013), all showing ~~synchronous~~ North Atlantic cooling beginning with the LSE (Figure ~~3~~4).~~-~~ S~~We note that s~~ome lacustrine proxy records suggest that cooling began a few years after the LSE (e.g., the Gerzensee $\delta^{18}O$ stack (van Raden et al., 2013)). The existence of a short lag between the temperature signal and the recording of that signal in some lacustrine archives may reflect differences in the type of archive used (i.e., terrestrial versus lacustrine archives~~, etc.~~), a decadal-scale residence time of groundwater feeding certain lakes, or differences in moisture source regions for different lakes. T~~We note that t~~he trigger responsible for the cooling must coincide with (or predate) the earliest evidence for the cooling, and a lag must logically exist between the forcing and any later response (rather than the effect preceding the cause).

Recent work used volcanic marker horizons to transfer the GICC05modelext to the GISP2 ice core (previously on the Meese/Sowers chronology) (Seierstad et al., 2014), thereby improving comparisons with records of volcanism (Figure 5~~4~~) and~~.~~ ~~This also~~ facilitating~~es~~ a direct comparison between the GISP2 volcanic sulphate record (Zielinski et al., 1997) and both the GISP2 and the NGRIP $\delta^{18}O$ records (Figure 5~~4~~). For the interval around the Younger Dryas initiation, very little chronological difference between this timescale and IntCal13 exists (Muscheler et al., 2014), suggesting the timescale is robust. ~~(MUSCHELRE et al., 14).~~ ~~This also facilitates a direct comparison between the GISP2 volcanic sulphate record (Zielinski et al., 1997) and both the GISP2 and the NGRIP~~ ~~$\delta^{18}O$ records.~~ We find that a large sulphate spike at 12.867 ka BP in the GISP2 record on the GICC05modelext timescale is contemporaneous~~-~~ with the LSE's timing based on *i*) ~~independent radiometricAr-Ar~~ dates for the eruption based on varved lake deposits (12.880 ± 0.05 ka BP (Lane et al., 2015)) and *ii*) layer counting from the Vedde Ash within NGRIP (Figure 5~~4~~); we therefore ascribe this sulphate spike to the Laacher See eruption. Earlier research also briefly considered this spike based on absolute dating (Brauer et al., 1999b), but because of the concept that the LSE preceded the YD boundary by ~200 years, concluded that an earlier sulphate spike most likely represented the LSE. Utilising the GICC05modelext chronology (Seierstad et al., 2014) as well as recent layer counts within the MFM sediment (Lane et al., 2013a), we now suggest that the sulphate spike at 12.867 ka BP ~~actually~~ represents the LSE, and that the earlier one tentatively identified by Brauer et al. (1999b) ~~{Lane, 2013 #2249}(1999) likely~~may represent~~s~~ a small eruption of the Icelandic volcano Hekla, consistent with the interpretation of Muschitiello et al. (2017)~~(2015)(2017)~~. Althoug~~T~~h~~e~~ the coincidence between our identified ~~this~~ sulphate spike, the date of the LSE, and the onset of North Atlantic cooling associated with the YD is ~~striking~~compelling, ~~.~~detailed tephrochronological analyses are required to definitively ascribe this sulphate spike to the LSE.

3.2 A complex response of climate to volcanic eruptions

Explosive volcanic eruptions can inject large amounts of sulphur-rich gases as either SO_2 or H_2S into the stratosphere, which are gradually ~~become~~ oxidised to form sulphuric acid vapour (Rampino and Self, 1984). Within a few weeks, the vapour can condense with water to form an aerosol haze (Robock, 2000), which is rapidly advected around the globe. Once in the atmosphere, sulphate aerosols induce summer cooling by scattering incoming solar radiation back to space (Timmreck and

Graf, 2006; Baldini et al., 2015a). An eruption's sulphur content, rather than its explosivity, determines most of the climate response (Robock and Mao, 1995; Sadler and Grattan, 1999). Furthermore, ~~R~~recent research has also highlighted the role of volcanogenic halogens, such as Cl, Br, and F, in depleting stratospheric ozone (Cadoux et al., 2015; Klobas et al., 2017; Kutterolf et al., 2013; Vidal et al., 2016; LeGrande et al., 2016). Ozone absorbs solar ultraviolet and thermal infrared radiation, and thus depletion in ozone can result in surface cooling, and would act to reinforce the radiative effects of volcanogenic sulphate aerosols (Cadoux et al., 2015).

Although the radiative effects associated with volcanic aerosols are reasonably well understood, the systematics of how volcanic eruptions affect atmospheric and oceanic circulation are less well constrained. Research suggests that volcanic eruptions affect a wide variety of atmospheric phenomenon, but the exact nature of these links remains unclear. For example, Pausata et al. (2015) ~~(2015)~~ used a climate model to conclude that high latitude NH eruptions trigger an El Niño event within 8-9 months by inducing a hemispheric temperature asymmetry leading to southward Intertropical Convergence Zone (ITCZ) migration and a restructuring of equatorial winds. The model also suggests that these eruptions could lead to AMOC shifts after several decades, consisting of an initial 25-year strengthening followed by a 35-year weakening, illustrating the potential for climate effects extending well beyond sulphate aerosol atmospheric residence times. ~~Quantifying the long-term influences of single volcanic eruptions is confounded by the effects of subsequent eruptions and other factors (e.g., solar variability), which can overprint more subtle feedbacks.~~Several modelling studies based on historical data suggest that eruptions may strengthen AMOC (Ottera et al., 2010; Swingedouw et al., 2014; Ding et al., 2014) ~~b~~ but also increase North Atlantic sea ice extent for decades to centuries following the eruption due to the albedo feedback and reductions in surface heat loss (Ding et al., 2014; Swingedouw et al., 2014)~~-~~. Other models suggest that AMOC may intensify initially, but then weaken after about a decade (Mignot et al., 2011)~~-~~. A modelling study by Schleussner and Feulner ~~(2013)~~ (2013) suggested that volcanic eruptions occurring during the last millennium increased Nordic Sea sea ice extent which weakened AMOC and eventually cooled the entire North Atlantic Basin. Importantly, Schleussner and Feulner (2013) ~~(2013)~~ concluded that short-lived volcanic aerosol forcings triggered "a cascade of sea ice-ocean feedbacks in the North Atlantic, ultimately leading to a persistent regime shift in the ocean circulation". Other research finds that North Atlantic sea ice growth following a negative forcing weakened oceanic convection and northward heat export during the Little Ice Age (Lehner et al., 2013). Quantifying the long-term influences of single volcanic eruptions is confounded by the effects of subsequent eruptions and other factors (e.g., solar variability, El Niño events), which can overprint more subtle feedbacks. For example, model results looking at recent eruptions found evidence that different types of eruptions can either constructively or destructively interfere with AMOC strength (Swingedouw et al., 2014)~~-~~. ~~North Atlantic sea ice ednorthwardheat (Lehner et al., 2013). Consequently~~Therefore, despite ~~recent~~ increasingly clear indications~~hints~~ that volcanic eruptions have considerable long-term consequences for atmospheric and oceanic circulation, the full scale of these shifts ~~are~~ is currently not well understood even over the last two millennia, and are essentially unknown under Glacial boundary conditions. ~~What is certain, however, is that existing climate modelling studies~~

~~do not exclude the possibility that the LSE triggered a positive feedback involving sea ice extension, AMOC weakening, and shifted atmospheric circulation.~~

However, i~~Despite these uncertainties, i~~ndications that volcanism may have had particularly long~~er~~-term climate effects during the last Glacial do exist. Bay et al. (2004) found a very strong statistical link between evidence for Southern Hemisphere (SH) eruptions and rapid Greenland warming associated with Dansgaard-Oeschger (DO) events, although no causal mechanism was proposed. More recent research determined that every large, radiometrically-dated SH eruption (five eruptions) across the interval 30-80 ka BP occurred within dating uncertainties of a DO event (Baldini et al., 2015a). The same research also found a strong statistical correlation between large NH volcanic eruptions and the onset of Greenland stadials over the same 30-80 ka BP~~-~~ interval (Baldini et al., 2015a), and proposed that during intermediate ice volume conditions, a positive feedback involving sea ice extension~~, glacier extent~~, atmospheric circulation shifts, and/or AMOC weakening may have amplified the initial aerosol injection. ~~The model suggests that this~~This positive feedback continued to operate until it was superseded by another external forcing promoting warming at high latitudes in the NH, or an equilibrium was reached. This ~~model~~ also appears consistent with observations during the YD: *i)* a ~~large~~ sulphur-rich NH volcanic eruption, *ii)* long-term NH high latitude cooling, *iii)* NH mid-latitude westerly wind migration to the south, and *iv)* slow recuperation out of the event following rising 65°N insolation and/or a SH meltwater pulse.

The LSE ~~produced~~erupted twice the volume of ~~was twice the magnitude~~~~tephra and lava~~magma as, and potentially injected ~~up to nine times the amount of~~considerably more SO_2 ~~sulphur~~ into the stratosphere than, ~~as~~ the 1991 AD Pinatubo eruption (Baales et al., 2002; Schmincke, 2004). The Pinatubo eruption cooled surface temperatures by 0.5°C globally, and up to 4°C ~~over~~across Greenland, Europe, and parts of North America, for two years following the eruption (Schmincke, 2004), while aerosols persisted in the stratosphere for ~~~four~~three years (Diallo et al., 2017). Existing models suggest that the LSE created a sulphate aerosol veil wrapping around the high northern latitudes (Graf and Timmreck, 2001) (Figure 3~~2~~). The model suggests that NH summer temperatures dropped by 0.4°C during the first summer following the eruption, though it assumes that the eruption released substantially less SO_2 ~~sulphur~~(15 Mt SO_2 (Graf and Timmreck, 2001)) than current maximum estimates[J3] (Figure 2)~~)~~ ~~(15 versus 150 megatons (Baales et al., 2002)~~ which rival the SO_2 amount delivered to the stratosphere by the 1815 Tambora eruption. Actual~~),(Graf and Timmreck, 2001), so actual~~ aerosol-induced cooling may far exceed these estimates, a perspective supported by Bogaard et al. (1989) who estimate that the environmental impacts of the eruption of the sulphur-rich phonolite melt may even have exceeded those of the far larger (but silicic) Minoan eruption of Santorini. ~~It is conceivable that this is reflecting the direct aerosol effects of the LSE.~~ .It does not seem unreasonable that an eruption of this size, sulphur content, and geographic location, ~~could~~ occurring during a transitional climate state, could have catalysed ~~a NH cooling event~~a significant climate anomaly.

3.3 The nature of the positive feedback

Volcanogenic sulphate aerosols typically settle out of the atmosphere within ~~five~~ three years; aerosol-induced cooling alone therefore cannot explain the YD's extended duration. We suggest that aerosol forcing related to the LSE initiated North Atlantic cooling, which consequently triggered a positive feedback as proposed for earlier Greenland stadials (Baldini et al., 2015a). Existing evidence strongly suggests that North Atlantic sea ice extent increased (Bakke et al., 2009; Baldini et al., 2015b; Broecker, 2006a; Cabedo-Sanz et al., 2013) and ~~-~~ AMOC weakened (Broecker, 2006a; McManus et al., 2004; Bondevik et al., 2006) ~~-~~immediately after the GS-1 onset, and therefore both could have provided a powerful feedback. The feedback may have resided entirely in the North Atlantic, and involved sea ice expansion, AMOC weakening, and increased albedo, as previously suggested within the context of meltwater forcing (Broecker et al., 2010). Alternatively, it has been noted that hemispherically asymmetrical volcanic sulphate loadings ~~may have~~ induced ITCZ migration away from the hemisphere of the eruption (Ridley et al., 2015; Hwang et al., 2013; Colose et al., 2016), and it is possible that these ITCZ shifts ~~may have~~ forced wholesale shifts in atmospheric circulation cells. This hypothesised mechanism is broadly consistent with that advanced by Chiang et al. (2014) where ~~the~~a ~~original~~ forcing ~~occurs~~ at a high northerly latitude ~~, but~~ subsequently drives southward ITCZ migration, which then affects global atmospheric circulation. Within the context of GS-1, LSE aerosol-related NH cooling could have shifted the ITCZ to the south, thereby expanding the NH Polar Cell and shifting the NH Polar Front to the south. Sea ice tracked the southward shifted Polar Front, resulting in more NH cooling, a weakened AMOC, ~~-~~ and a further southward shift in global atmospheric circulation cells (Baldini et al., 2015a). Such a scenario is consistent with recent results based on tree ring radiocarbon measurements suggesting that GS-1 was not caused exclusively by long-term AMOC weakening, but instead was forced by NH Polar Cell expansion and southward NH Polar Front migration (Hogg et al., 2016).

Our hypothesis that the YD was triggered by the LSE and amplified by a positive feedback is further supported by modelling results suggesting that a combination of a moderate negative radiative cooling, AMOC weakening, and altered atmospheric circulation best explain the YD (Renssen et al., 2015). ~~Furthermore,~~ AMOC consists of both thermohaline and wind-driven components, and atmospheric circulation changes can therefore dramatically affect oceanic advection of warm water to the North Atlantic. Recent modelling suggests that reduced wind stress can immediately weaken AMOC, encouraging southward sea ice expansion and promoting cooling (Yang et al., 2016), illustrating a potential amplification mechanism following an initial aerosol-induced atmospheric circulation shift. Twentieth ~~20th C~~Century instrumental measurements further support this by demonstrating that westerly winds strength over the North Atlantic partially modulates AMOC (Delworth et al., 2016).

~~Initiation of the positive feedback requires volcanic aerosols to remain in the atmosphere for at least one summer season. E (Schmincke et al., 1999), and varve studies similarly suggest a late spring/early summer eruption (Merkt and Muller, 1999){Baales, 2002 #2251}For historical eruptions similar in size to the Laacher See eruption, aerosols have remained in the atmosphere far longer than one year, regardless of the eruption's latitude. For example, aerosols remained in the atmosphere for ~three years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which is estimated to have released approximately considerably less sulphur than the LSE (~15-20 Mt (Sheng et al., 2015) versus (Textor et al., 2003b)potentially~~

up to 150 Mt (Baales et al., 2002))(Sheng et al., 2015). Aerosols remained in the atmosphere for approximately three years even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected only 2.1 Mt SO_2 (Baales et al., 2002; Pitari et al., 2016) and erupted laterally (Eychenne et al., 2015). It is worth noting that aerosols in the atmosphere during the winter increase winter temperatures (Kirchner et al., 1999), which would promote ice sheet growth. In short, volcanic eruptions are ideal for triggering ice growth, and even if the eruption were a winter eruption, the LSE's aerosols would have certainly persisted over at least the following summer, with the potential to catalyse the positive feedback we invoke. Initiation of the positive feedback requires volcanic aerosols to remain in the atmosphere for at least one summer season. Evidence based on the seasonal development of vegetation covered by the LST suggests that the LSE occurred during late spring or early summer (Schmincke et al., 1999), and varve studies similarly suggest a late spring or early summer eruption (Merkt and Muller, 1999). Available evidence therefore suggests that the eruption occurred just prior to maximum summer insolation values, maximising the potential scattering effects of the volcanogenic sulphate aerosols. Even if it were a winter eruption, for historical eruptions similar in magnitude to the Laacher See eruption, aerosols remained in the atmosphere longer than one year, regardless of the eruption's latitude. For example, aerosols remained in the atmosphere for ~three years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which probably released considerably less SO_2 than the LSE (Figure 2). Measurable quantities of aerosols remained in the atmosphere for approximately three years even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected only 2.1 Mt SO_2 (Baales et al., 2002; Pitari et al., 2016) and erupted laterally (Eychenne et al., 2015). In short, the LSE eruption probably occurred during the late spring or early summer, but even if the eruption were a winter eruption, the LSE's aerosols would have certainly persisted over at least the following summer, with the potential to catalyse the positive feedback we invoke.

It is worth highlighting that

Aa similar mechanism may have also contributed to Little Ice Age cooling (Miller et al., 2012), with recent research suggesting that a coupled sea ice/AMOC mechanism could extend the cooling effects of volcanic aerosols by over 100 years during the Little Ice Age (Zhong et al., 2011). This perspective is supported by modelling results suggesting that a large volcanic forcing is required to explain Little Ice Age cooling (Slawinska and Robock, 2017). Lehner et al. (2013)– identify a sea ice/AMOC/atmospheric feedback that amplified an initial negative radiative forcing to produce the temperature pattern characterising the Little Ice Age. Rahmstorf et al. (2015) argue that no identifiable change in AMOC occurred during the Little Ice Age, suggesting that the drivers were related to sea ice and atmospheric rather than oceanic, consistent with the perspective that large volcanic eruptions were responsible for this centennial-scale cooling eventalthough this requires further research to confirm. (2015) LSimilarly, large volcanic eruptions in 536, 540, and 547 AD are hypothesised to have triggered a coupled sea ice/AMOC feedback that led to leading to an extended cold period (Buntgen et al., 2016). Recent research also highlights the possibility that volcanism followed by a coupled sea ice/ocean circulation positive feedback triggered hemispheric-wide centennial to millennial-scale variability during the Holocene (Kobashi et al., 2017)., If a feedback was active following volcanic eruptions during the 6th Century, the Little Ice Age, and the Holocene, the intermediate ice volume and transitional climate characteristic of the last deglaciation should have amplified their effects. This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated "Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval." Our results are entirely consistent with this perspective, and here we ~~identify~~highlight a sulphur-rich volcanic eruption ~~potentially responsible for the YD~~whose timing coincided with the onset of YD-related cooling.

~~Our hypothesis that the YD was triggered by the LSE and amplified by a positive feedback is further supported by modelling results suggesting that a combination of a moderate negative radiative cooling, AMOC weakening, and altered atmospheric circulation best explain the YD (Renssen et al., 2015). Furthermore, AMOC consists of both thermohaline and wind-driven components, and atmospheric circulation changes can therefore dramatically affect oceanic advection of warm water to the North Atlantic. Recent modelling suggests that reduced wind stress can immediately weaken AMOC, encouraging southward sea ice expansion and promoting cooling (Yang et al., 2016), illustrating a potential amplification mechanism following an initial aerosol-induced atmospheric circulation shift. 20th Century instrumental measurements further support this by demonstrating that westerly winds strength over the North Atlantic partially modulates AMOC (Delworth et al., 2016).~~ However, despite increasingly tangible evidence that eruptions ~~trigger atmospheric circulation shifts that~~ can ~~subsequently~~ affect AMOC strength and sea ice extent, the exact nature of any positive feedback is still unclear. Future research should prioritize the identification and characterisation of this elusive, but potentially commonplace, feedback that amplifies otherwise subtle NH temperature shifts.

3.4 Sensitivity to ice volume

Magnitude 6 (M6) eruptions are large but not rare; for example, over the last two thousand years, twelve M6 or larger eruptions ~~occurred~~ are known (Brown et al., 2014), but none produced a cooling event as pronounced as the YD. Abrupt millennial scale climate change is characteristic of glacial intervals, and the forcing responsible only appears to operate during intervals when large ice sheets are present. The lack of large-scale and prolonged climate response to external forcing over the Holocene implies either the absence of the forcing (e.g., lack of large meltwater pulses) or a reduced sensitivity to a temporally persistent forcing (e.g., volcanism). The subdued climate response to volcanic eruptions over the recent past could reflect low ice volume conditions and the absence (or muting) of the requisite positive feedback mechanism. However, millennial-scale climate change was also notably absent during the Last Glacial Maximum (~20-30 ka BP), implying that very large ice sheets also discourage millennial-scale climate shifts. The apparent high sensitivity of the climate system to millennial-scale climate change during times of intermediate ice volume is well documented (e.g., Zhang et al., 2014; Zhang et al., 2017). .

The apparent high sensitivity of the climate system to millennial-scale climate change during times of intermediate ice volume is well documented (e.g., Zhang et al., 2014; Zhang et al., 2017), and here we Here, we investigate this further by examininge the timing of Greenland Stadials relative to ice volume (estimated using Red Sea sea level (Siddall et al., 2003)). The timings of 55 stadial initiations as compiled in the INTIMATE (INTegration of Ice core, MArine, and TErrestrial) initiative initiative (Rasmussen et al., 2014) are compared relative to ice volume, and indeed a strong bias towards intermediate ice volume conditions exists, with 73% of the millennial scale cooling events occurring during only 40% of the range of sea level across the interval from 0-120 ka BP (Figure 56). The A Gaussian distribution of events exists, withsuggests that the most sensitive conditions are linked to ice volume associated with a sea level of -68.30 m below modern sea level. This intermediate ice volume was commonplace from 35-60 ka BP, and particularly from 50-60 ka BP. However, over the interval from 0-35 ka BP, these ideal intermediate ice volume conditions only existed during a short interval from 11.8-13.7 ka BP, and optimal conditions were centred around at 13.0 ka BP (Figure 65).. These results are broadly consistent with previous research (e.g., Zhang et al., 2014; Zhang et al., 2017), but- the timing and duration of the most sensitive interval of time, and the likelihood that a forcing produces a longer-term cooling event, may ultimately depend on a more complexit is also possible that interplay between ice volume and atmospheric CO_2 determines the timing and duration of the most sensitive interval of time, and the likelihood that a forcing produces a longer-term cooling event (Zhang et al., 2017). Atmospheric pCO_2 during the YD initiation was relatively high (~240 ppmv) and could therefore affect the timing of ideal conditions for abrupt climate change in conjunction with ice volume, but their precise interdependence is still unclear. AdditionallyFinally, a frequency distribution of the sea level change rate associated with each stadial indicates that whatever mechanism is responsible for triggering a stadial operates irrespective of whether sea level (i.e., ice volume) is increasing or decreasing (Figure 65b). Because active ice sheet growth should not promotediscourage meltwater pulses, this observation seemingly argues against meltwater pulses as the sole trigger for initiating stadials. It is not clear what role,

3.5. Comparison with the climate response to the Toba supereruption

CIt is useful to comparinge the LSE to another well-dated Quaternary eruption occurring under different background conditions provides information regarding the nature of the relevant forcings. The magnitude -> 8 (i.e., ~100x greater than the LSE) Toba supereruption occurred approximately ~74 ka BP ,and ,was the largest eruption of the Quaternary (Brown et al., 2014). Despite its size, the climate response to the eruption remains vigorously debated, with some researchers suggesting that the eruption triggered the transition to Greenland Stadial 20 (-GS-20) (Polyak et al., 2017; Baldini et al., 2015a; Carolin et al., 2013; Williams et al., 2009) but others arguing for only a small climate response (Haslam and Petraglia, 2010)). However, it does not appear that the eruption triggered a sustained global volcanic winter (Lane et al., 2013b; Svensson et al., 2013)(Lane et al., 2013b)(Lane et al., 2013b(Svensson et al., 2013))(Svensson et al., 2013), and therefore globally homogenous long-term aerosol induced cooling probably did not occur. The Toba supereruption occurred during an orbitally-modulated cooling trend with intermediate ice volume (-71 m below modern sea level, very near the sea level associated with the most sensitive ice volume conditions (-68.3 m below modern sea level, as suggested here)), and it is possible that the eruption expedited the transition to GS-20 through a combination of an initial short-lived aerosol-induced cooling amplified by a positive feedback lasting hundreds of years. ~~The high latitude NH cooling was accompanied by southward ITCZ migration and Antarctic warming, consistent with sea ice growth across the North Atlantic and a weakened AMOC. (Mark et al., 2017; Storey et al., 2012)(2013) the timing remains uncertain. I~~

In this analysis we use the timing of the Toba eruption relative to the NGRIP record suggested by Svensson et al., (2013) (their NGRIP sulphate spike 'T2'), but the timing remains uncertain. If future research revises the timing relative to NGRIP, this would necessarily affect the interpretations discussed below. Assuming the Toba eruption was responsible for the sulphate spike T2, the eruption was followed by high latitude NH cooling accompanied by southward ITCZ migration and Antarctic warming, consistent with sea ice growth across the North Atlantic and a weakened AMOC. The rates of cooling into GS-1 (the YD) after the LSE and GS-20 after the Toba eruption are nearly identical (Figure 7~~6~~), ~~implying~~ consistent with~~that~~ the possibility that a similar process was responsible for the cooling in both instances. The cooling rate may reflect the nature of the post-eruptive positive feedback, ~~and~~ which ~~was~~ was potentially~~largely~~ independent of the initial radiative cooling effects of the two eruptions, provided that they were large enough to trigger a feedback. The reconstructed climate responses following the LSE and Toba eruptions diverge after the achievement of maximum cooling. In the case of Toba, cold conditions persisted for ~1.7 ka before the very rapid temperature increase characteristic of the onset of Greenland Interstadial (an abrupt Greenland warming event) 19 (GI-19) (Figure 7~~6~~). GS-1 ended after ~1.2 ka, but unlike GS-20 ~~,~~ which had stable cold temperatures throughout, based on available $\delta^{18}O$ (Baldini et al., 2015b; Steffensen et al., 2008) and nitrogen isotope data (Buizert et al., 2014), the coldest conditions ~~i~~around the North Atlantic ~~n Greenland~~ occurred near the beginning of the GS-1~~-~~ and were followed by gradual warming ~~(Litt et al., 2003)~~. The LSE and Toba eruptions occurred under different orbital configurations, and the contrasting topologies characteristic of GS-1 and GS-20 may reflect differing insolation trends. At ~13 ka BP, summer insolation at 65°N was increasing, rather than decreasing as during the Toba eruption ~74 ka BP. The Toba eruption may have ~~catalysed~~ triggered a shift to the insolation-mediated baseline (cold) state, whereas the LSE may have ~~forced~~ catalysed a temporary shift to cold conditions opposed to the insolation-driven warming trend~~-~~ characteristic of that time interval, resulting in a short-lived cold event followed by gradual warming. Radiative cooling events in the Southern Hemisphere (e.g., a SH eruption, an Antarctic meltwater pulse, etc.) may have abruptly terminated both GS-1 and GS-20; the GS-1 termination is broadly consistent~~coincides~~ with the putative timing of Meltwater Pulse 1B (MWP-1B) (Ridgwell et al., 2012) ~~(Leventer et al., 2006; Ridgwell et al., 2012)~~, whereas any SH trigger for the termination of GS-20 is unidentified. MWP-1B (or another SH cooling event) may have cooled the SH and strengthened AMOC, prompting northward migration of the ITCZ and NH mid-latitude westerlies to achieve equilibrium with high insolation conditions, thereby rapidly reducing sea ice extent and warming Greenland, but this requires further research, particularly because the source, duration, timing, and even existence of

MWP-1B are still debated. R~~-~~educed oceanic salt export within the North Atlantic subtropical gyre, ~~, as is~~ characteristic of stadials, may have preconditioned the North Atlantic toward vigorous AMOC following the initial migration of atmospheric circulation back to the north (Schmidt et al., 2006).

The residence time of aerosols within the atmosphere is not critical within the context of this model provided the positive feedback is activated, and~~. A~~ a sufficiently high aerosol-related cooling over ~~even~~ only one summer and one hemisphere could suffice. The strength of the feedback may also depend on the amount of hemispheric temperature asymmetry caused by the eruption. Consequently, the high latitude LSE may actually have induced a stronger hemispheric temperature asymmetry than the low latitude Toba eruption, although the Toba eruption would have resulted in considerably more overall cooling. The long-term (e.g., hundreds to thousands of years) climate response will depend on the climate background conditions; if a NH eruption occurs during an orbitally-induced cooling trend (as ~~was~~may have been~~ apparently~~ the case for Toba), the eruption will catalyse cooling towards the insolation-mediated baseline. If the NH eruption occurs during rising insolation and intermediate ice volume (e.g., Laacher See), the eruption will trigger the feedback which will continue until it is overcome by a sufficiently high positive insolation forcing, or a SH cooling event. Although the size of the two eruptions differed by two orders of magnitude, the residence time of the respective aerosols in the stratosphere was probably broadly similar: ~three~~four~~ years for the LSE (similar to that of the Pinatubo eruption) and ~10 years for Toba (Timmreck et al., 2012; Robock et al., 2009). Although modelling results suggest that Toba resulted in ~10 degrees C of cooling (Timmreck et al., 2012), compared to probably around ~1 degree Celsius for the LSE, we argue that both eruptions were able to cool climate ~~enough~~sufficiently to trigger sea ice growth, weaken AMOC, and thereby ~~start~~initiate the positive feedback.

~~-~~A useful way of visualising this is to consider two extreme scenarios: *i*) very high CO_2 concentrations and no ice (e.g., the Cretaceous) and *ii*) very low CO_2, low insolation, and very high ice volume (e.g., the Last Glacial Maximum). An eruption the size of the LSE would probably not significantly affect climate beyond the atmospheric residence time of the sulphate aerosols during either scenario. Under the background conditions characteristic of the first scenario, insufficient aerosol forcing would occur to trigger ice growth anywhere, and consequently no positive feedback would result. In the case of the second scenario, the eruption would cause ice growth and cooling for the lifetime of aerosols in atmosphere before conditions returned to the insolation-mediated equilibrium~~baseline~~. In contrast to these extreme scenarios, an injection of volcanogenic sulphate aerosols into the NH atmosphere would most effectively trigger a feedback during intermediate CO_2 ~~(~~and consequently ice volume~~)~~ conditions; in other words, during a transition from one ice volume state to another when the climate system was in a state of disequilibrium. Under these conditions, we suggest that activation of the feedback would occur even if the sulphate aerosols settled out of the atmosphere after just one year. Therefore, although the Toba and Laacher See eruptions were of very different magnitudes, the nature of the positive feedback would depend largely on the background conditions present during the individual eruptions. We argue that both eruptions were large enough, and contained enough sulphur, to activate the positive feedback under their respective background conditions. The strength of the positive feedback was then controlled by background conditions rather than eruption magnitude~~the size of the eruptions~~ (or amount of SO_2 released). Furthermore, we predict that the more asymmetric the hemispheric distribution of the aerosols, the stronger the feedback. For these reasons, the long-term (centennial- to millennial-scale) climate response following the extremely large but low latitude Toba eruption ~~would~~may have approximated those of the far smaller but high latitude Laacher See eruption. ~~It~~Therefore, ~~is also worth noting that~~ the long-term climate repercussions to ~~a~~ very large volcanic eruptions may not consist of prolonged~~long term~~ radiative cooling (i.e., 'volcanic winter') but rather of geographically disparate dynamical shifts potentially not proportional to eruption magnitude.

3.6 Response to other late Quaternary eruptions

The strongest argument against the LSE contributing to YD cooling is that several other similar or larger magnitude eruptions must have occurred during the last deglaciation, and that therefore the LSE was not unusual. However, the LSE *i)* was unusually sulphur-rich, *ii)* was high-latitude, and *iii)* coincided with ideal ice volume conditions. It is in fact the only known sulphur-rich, high latitude eruption coinciding with the most sensitive ice volume conditions during the last deglaciation. Early work noted that because the 1963 Agung eruption was very sulphur-rich, it forced cooling of a similar magnitude as the 1815 Tambora eruption, despite Tambora ejecting considerably more fine ash (~150 times more) into the upper atmosphere (Rampino and Self, 1982). Although estimates of total sulphur released vary, it is clear that the LSE was also unusually sulphur-rich (releasing more SO_2 than Agung (Figure 2)), so had the potential to exert a considerable forcing on climate disproportionate to its ~~size~~magnitude. Furthermore, other eruptions may also have contributed to climate change but current chronological uncertainties preclude establishing definitive links. For example, the ~14.2 ka BP Campi Flegrei eruption may have caused cooling (potentially related to the Older Dryas~~cooling~~ at ~14 ka BP) but this is ~~currently not~~impossible to confirm with eruption age uncertainties of ±1.19 ka.

Other large sulphate spikes exist in the GISP2 sulphate record, and in fact the two spikes at 12.6 ka BP and 13.03 ka BP are both larger than the potential LSE sulphate spike. The size of the sulphate spike is not necessarily proportional to the size of the eruption, and the sulphate spike at 13.03 ka BP is likely related to the small eruption of Hekla (Iceland) (Muschitiello et al., 2017), which deposited sulphate in the nearby Greenland ice but was climatologically insignificant. Similarly, the spike at 12.6 ka BP may also reflect the influence of a small but proximal unknown eruption. ~~It should~~ Alternatively, a large eruption of Nevado de Toluca, Mexico, is dated at 12.45 ± 0.35 ka BP (Arce et al., 2003), and could correspond to the large sulphate spike at 12.6 ka BP within the GISP2 ice core. The eruption was approximately the same size as the LSE, so the lack of long-term ~~climate~~ cooling may reflect a different climate response due to the eruption's latitude, which caused a more even distribution of aerosols across both hemispheres~~, or a lower sulphur load~~. ~~Also possible is that the spike does not reflect the Nevado de Toluca eruption, which but another smaller, but more proximal eruption.~~ Although there is no long-term cooling following the 12.6 ka BP sulphate spike, ~~T~~i~~the 12.6 ka BP sulphate spike~~ is associated with a short ~~but dramatic~~ cooling event; therefore the lack of long-term cooling may ~~simply~~ reflect the fact that temperatures had already reached the lowest values possible under the insolation and carbon dioxide baseline conditions characteristic of that time. ~~The sulphate spike at 13.03 ka BP is likely related to the small eruption of Hekla (Iceland) (Muschitiello et al., 2017), which deposited sulphate in the nearby Greenland ice but was climatologically insignificant.~~

3.7 Compatibility with other hypotheses

A substantial amount of research has now characterised the YD climate, and several hypotheses exist attempting to explain the excursion. The most well-researched hypothesis involves a freshwater pulse from the large proglacial Lake Agassiz severally reducing AMOC strength (Alley, 2000; Broecker, 1990; Broecker et al., 2010). Weakened AMOC would result in a colder North Atlantic, a southward displaced ITCZ, and Antarctic warming, all features characteristic of the YD. However, direct geological evidence for a meltwater pulse originating from Laurentide proglacial lakes coincident with the YD onset remains elusive (Broecker et al., 2010; Petaev et al., 2013). ~~although r~~Recent research suggests that meltwater from Lake Agassiz could have had an ice-free route to the North Atlantic during YD initiation ~~(Leydet et al., 2018)~~(Leydet et al., 2018), revising earlier ~~with~~ ice sheet reconstructions suggesting that the Laurentide Ice Sheet blocked the most obvious meltwater pathways to the North Atlantic (Lowell et al., 2005)~~, but~~. Although direct evidence that a meltwater pulse occurred at the onset of the YD along this route is limited~~. However, the issue remains controversial, with~~ some sediment core evidence suggesting that floods through the St Lawrence Valley ~~and into the North Atlantic did in fact~~ coincidede with the YD initiation ~~(Levac et al., 2015; Rayburn et al., 2011)~~ does exist (Levac et al., 2015; Rayburn et al., 2011). A northward meltwater flowpath along the MacKenzie Valley to the Arctic Ocean is also possible (Murton et al., 2010; Not and Hillaire-Marcel, 2012; Tarasov and Peltier, 2005). This is supported by both geological and modelling evidence, which suggests that a freshwater pulse into the Arctic Ocean could have weakened AMOC by >30%, considerably more that the <15% resulting from a similar pulse injected into the North Atlantic (Condron and Winsor, 2012). ~~However, the issue remains controversial, with some evidence suggesting that floods through the St Lawrence Valley and into the North Atlantic did in fact coincide with the YD initiation (Levac et al., 2015; Rayburn et al., 2011).~~ Still other evidence implicates meltwater from the Fennoscandian Ice Sheet (FIS) as the YD trigger (Muschitiello et al., 2015; Muschitiello et al., 2016), highlighting meltwater provenance as a key uncertainty intrinsic to the meltwater hypothesis. Intriguingly, Muschitiello et al. (2015) find that the FIS-derived meltwater pulse ended at 12.880 ka BP, ~~perfectly~~ coinciding ~~synchronous~~ with the LSE. It is therefore possible that the meltwater pulse resulted from insolation-controlled melting of the FIS, until the cooling associated with the LSE and associated positive feedback reversed this trend, though ~~this is~~ currently this remains speculative. Finally, evidence from a sediment site SW of Iceland very strongly suggests that cooling into stadials precedes evidence for iceberg discharges, and that therefore North Atlantic iceberg-related surface water freshening was not the trigger for stadial events (Barker et al., 2015). D~~(Brauer et al., 2008; Wunsch, 2006)(Brauer et al., 2008)~~espite these issues, a meltwater pulse is still widely considered as the leading hypothesis for the initiation of the YD, although the meltwater source~~, and~~ destination, and duration are still vigorously debated.

Iridium-rich magnetic grains, nanodiamonds, and carbon spherules coinciding with a carbon-rich black layer at approximately 12.9 ka BP were interpreted by Firestone et al. (2007) as evidence that an extraterrestrial impact (or impacts) may have contributed to the YD. The consequences of the impact (possibly a cometary airburst) were hypothesised to have destabilised the Laurentide Ice Sheet, cooled NH climate, and contributed to megafaunal extinction characteristic of the period (Firestone et al., 2007; Kennett et al., 2009). The ~~recent~~ discovery of significant amounts of impact-derived spherules scattered across North America, Europe, Africa, and South America at ~12.80 ± 0.15 ka BP ~~provides~~ further supports ~~for~~ the Younger Dryas Impact Hypothesis (YDIH) (Wittke et al., 2013), as does apparently extraterrestrially-derived platinum, found initially in Greenland ice (Petaev et al., 2013) and subsequently globally (Moore et al., 2017), coincident with the YD onset~~(Petaev et al., 2013a)~~. However, other research questions the ~~interpretations~~evidence for an impact, focussing on perceived errors in dating of the YD boundary layer (Holliday, 2015; Meltzer et al., 2014), misidentification of terrestrially-derived carbon spherules and shocked quartz as extraterrestrial (Pinter et al., 2011; van Hoesel et al., 2015), non-uniqueness of YD nanodiamond evidence (Daulton et al., 2017), and inconsistencies regarding the physics of bolide trajectories and impacts (Boslough et al., 2013). Despite these criticisms, independent researchers have linked spherules found within the YD boundary layer to an impact (LeCompte et al., 2012), and identified the source of the spherules as northeastern North America (Wu et al., 2013), not only supporting the hypothesis, but also suggesting an impact site proximal to the southern margin of the Laurentide Ice Sheet (Wu et al., 2013). Most recently, Wolbach et al. ~~(2018)~~ (2018a, 2018b) catalogue extensive evidence for global wildfires they argue were ignited by the impact of fragments of a >100 km diameter comet. According to Wolbach et al. (2018a, 2018b)~~)~~, 9% of global biomass burned, considerably more than that following the end of Cretaceous impact event. Despite this~~a~~ growing catalogue of evidence for an impact event, the YDIH remains controversial. Possibly due to this controversy, the majority of research relating to the YDIH has focussed on verifying the evidence for an impact, and the possible climate repercussions stemming from an impact remain relatively poorly constrained.

Of these proposed triggers, only the Laacher See eruption is universally accepted as having occurred near the YD/Allerød boundary, and strong evidence now exists dating the eruption precisely at the onset of YD-related North Atlantic cooling (GS-1). However, we emphasise that these hypotheses are not mutually exclusive. For example, meltwater releases into the North Atlantic and Arctic Ocean undoubtedly did occur during the last deglaciation, and a meltwater pulse and a volcanic eruption occurring within a short time of each other could conceivably result in an amplified climate response. Similarly, available evidence suggests that an impact event ~~likely~~may have occurred within a few decades of the LSE, and in fact Wolbach et al. (2018) date the impact to 12.854 ± 0.056 ka BP based on a Bayesian analysis of 157 dated records interpreted as containing evidence for the impact. This date is just 26 years after the 12.880 ka BP LSE (and well within dating uncertainty)~~,~~ and the two events could therefore have both contributed to environmental change associated with the YD. Conversely, the LSE could also be directly responsible for some of the evidence for an impact, a possibility that has not been investigated thoroughly due to the widespread, but incorrect, concept that the eruption predates the evidence for an impact by ~200 years. An impact event (independently or in conjunction with YD cooling) is suggested to ~~could partially~~ explain North American megafaunal extinctions (Firestone et al., 2007; Wolbach et al., 2018a; Wolbach et al., 2018b), though ~~we note that~~ recent research has built a compelling case that anthropogenic factors such as overhunting and disease were largely responsible for the demise of many species of large mammalian fauna (Sandom et al., 2014; Bartlett et al., 2016; van der Kaars et al., 2017; Cooper et al., 2015; Metcalf et al., 2016). However, even if future research determines that megafaunal extinctions were caused by human activity rather than a bolide impact, this does not reduce the YDIH's explanatory power as a YD trigger. ~~Furthermore~~Again, it is conceivable that the LSE, a bolide impact, and/or a meltwater pulse occurred within a short interval, reinforcing each other. A similar eruption-impact mechanism has been proposed for major mass extinctions through geological time ~~-~~(Tobin, 2017; White and Saunders, 2005; Keller, 2005), but these involved long-term flood basalt (e.g., the Deccan Traps) emplacement rather than a single, short-lived eruption. The probability of multiple triggers occurring within a short time of each other is low, but cannot be excluded outright ~~outright~~.

A key ~~n~~ issue with the YDIH however is that YD-like events ~~exist~~apparently occurred during previous terminations, such as TIII, (Broecker et al., 2010; Cheng et al., 2009; Sima et al., 2004) that are not reasonably attributable to other extraterrestrial impacts. However, TIII would also likely have experienced at least one high latitude Northern Hemisphere M > 6 volcanic eruption. Termination II lacked a YD-like event, possibly because extremely rapid sea level rise (and ice volume decreases) and very high insolation levels (Carlson, 2008) may have discouraged a strong post-eruptive positive feedback and/or shortened the 'window' of ideal ice volume conditions. Another issue with the YDIH is that the YD was simply not that anomalous of a cold event, and therefore does not require an unusually powerful trigger. Over the last 120 ka, 26 Greenland Stadial (GS) events occurred (Rasmussen et al., 2014), of which the YD was the most recent. This does not exclude the possibility that the YD was forced by an impact event, but the most parsimonious explanation is that most stadial events had similar origins, implying a much more commonplace trigger than an impact. Furthermore, nitrogen isotopes suggest that Greenland temperature was 4.5ºC colder during the Oldest Dryas (18 to 14.7 ka BP) than the YD (Buizert et al., 2014), again suggesting the transition to the YD does not require an extreme forcing. It is also worth noting that the YD may actually represent a return to the insolation-controlled baseline, and that potentially the Bølling-Allerød (B-A) warm interstadial (i.e., GI-1, from 14.642 to 12.846 ka BP), immediately preceding the YD, was the anomaly (Thornalley et al., 2011; Sima et al., 2004). Although an in-depth discussion of the B-A is outside the scope of this study, the B-A may represent an interval with a temporarily invigorated AMOC (an 'overshoot') (Barker et al., 2010), which, after reaching peak strength, began to slow down back towards its glacial state because of the lack of a concomitant rise in insolation (Knorr and Lohmann, 2007; Thornalley et al., 2011). The rate of this slowdown was independent of the final trigger into the YD, and indeed much of the cooling back to the glacial baseline was achieved by ~13 ka BP. Consequently, only a small 'nudge' may have been required to expedite the return to the cold baseline state, consistent with a very sulphur-rich volcanic eruption (occurring every few hundred years) but not necessarily with a rare, high-consequence event. In other words, the B-A may represent the transient ~~transient~~'anomaly~~ous'~~, and the conditions within the YD ~~was~~ represented typical near-glacial conditions, obviating the need for an extreme YD triggering mechanism. The cooling after the B-A cannot account for the rapid cooling observed ~~Although gradual cooling back~~

towards near-glacial conditions followed the B-A, the rate of cooling increases dramatically at around 12.9 ka BP clearly visible in the NGRIP $\delta^{18}O$ and nitrogen isotope data as well as in numerous other North Atlantic records, indicating that another forcing expedited the final cooling into the YD, which we argue was the LSE.

(Barker et al., 2010)A possible scenario is therefore therefore that many glacial terminations were characterised by B-A like events that were then followed by gradual cooling back to near-glacial conditions. This gradual cooling was sometimes interrupted and accelerated by -YD-like events forced by high latitude volcanism, possibly linked to crustal stresses induced by deglaciation (Zielinski et al., 1996). Some deglaciations did not experience a YD-like event due to , but others were not due to short-lived ideal ice volume conditions not coinciding with an eruption. The We stress however that deglaciations were also characterised by large meltwater pulses, and consequently the presence of a YD-like event during multiple deglaciations is consisted with either mechanism.

4 Conclusions

WWe suggest propose that the unusually sulphur-rich ~12.9 880 ± 0.040 kaka BP Laacher See volcanic eruption may have contributed triggeredtocontributed to GS-1 cooling and the atmospheric reorganisation associated with the YD event (beginning at 12.870 ± 0.138 ka BP in NGRIP; GS-1 is defined as starting at 12.846 ka BP, but cooling began somewhat earlier).. Recent revisions to the chronological framework of key European climate archives (e.g., the Meerfelder Maar sediment cores) now strongly suggest that the onset of GS-1 related North Atlantic cooling -occurred simultaneously with the LSE. WNotably, we have identified a large volcanic sulphur spike within the GISP2 ion data (Zielinski et al., 1997) on the new recent GICC05modelext chronology that coincides with both the timing of the LSE and the onset of GS-1 cooling within the same core. Lipid biomarker hydrogen isotope ratios from Lake Meerfelder Maar further corroborate that GS-1 atmospheric cooling began at 12.880 ka BP, coincident with the Laacher See Tephra within the same sediment and but preceding the larger dynamical atmospheric response associated with the YD in central Europe by ~170 years (Rach et al., 2014). Aerosol induced cooling following the eruption may have triggered a positive feedback involving sea ice expansion and/or AMOC weakening, as previously proposed for other Greenland stadials over the interval 30-80 ka BP (Baldini et al., 2015a), the 6th Century AD (Buntgen et al., 2016) (Buntgen et al., 2016), the Little Ice Age (Zhong et al., 2011; Miller et al., 2012) (Zhong et al., 2011;Miller et al., 2012), and the Holocene (Kobashi et al., 2017) (Kobashi et al., 2017). Viewed from this perspective, the YD was simply the latest, and last, manifestation of a Last Glacial stadial..

The strongly asymmetric nature of the sulphate veil released by the LSE cooled the Northern Hemisphere preferentially, inducing a strong hemispheric temperature asymmetry and triggering a cascade of dynamical climate shifts across both hemispheres, including a southward shifted ITCZ and Hadley Cells. Intermediate ice volume conditions around ~13 ka BP, driven by rising insolation during the Last Glacial termination, may have promoted a positive feedback following the LSE's injection of ~~up to 150~~between 6.76 and 104.8 megatonnes of SO_2 ~~sulphur~~ into the stratosphere (Textor et al., 2003). This is also consistent with observations that YD-like events were not unique to the last deglaciation, but existed during older deglaciations as well (Broecker et al., 2010). At least one high-latitude M6 eruption likely occurred during most deglacial intervals, and therefore~~.~~ GS-1 and earlier stadials may simply ~~therefore~~ reflect the convergence of ~~a~~ large, sulphate-rich high latitude NH eruptions with intermediate ice volume conditions. NH continental ice sheet decay induced continental lithospheric unloading, and may have triggered high latitude NH volcanism (Zielinski et al., 1997; Sternai et al., 2016), ~~introducing~~ highlighting the intriguing possibility that eruptions such as the LSE were not randomly distributed geographically and temporally, but instead were intrinsically linked to deglaciation. This perspective is strongly supported by a previous observation that the three largest eruptions (including the LSE) in the eastern Eifel volcanic field (Germany) were all associated with warming during glacial terminations and the reduction in ice mass in northern Europe (Nowell et al., 2006).

The hypothesis that the Laacher See eruption triggered the YD ~~hypothesis~~ is testable. Detailed tephrochronological studies of the ice containing the sulphate spike identified here could confirm the source of the sulphate, and, if it is confirmed as the LSE rather than a smaller Icelandic eruption, this would provide an important step towards attributing ~~the~~ GS-1 and the YD to volcanic forcing (although GS-1 cooling already seems to occur immediately above the LST in central Europe). Similarly, volcanic sulphate 'triple' isotope ratios of sulphur and oxygen provide information regarding the residence time of volcanic plumes in the stratosphere. The majority of atmospheric processes encourage mass dependent fractionation; however rare mass-independent fractionation processes produce isotope ratios that do not behave according to predictions based on mass dependent processes (Martin et al., 2014). Historical volcanic eruptions where sulphate aerosols reached the stratosphere have been successfully identified in ice cores (Baroni et al., 2008; Savarino et al., 2003), indicating that the technique is effective at distinguishing large explosive eruptions from smaller local ones. This technique could also determine if the sulphate in the potential LSE sulphate spike reached the stratosphere. Although this would not necessarily confirm the LSE as the source, it would strongly suggest that the sulphate was sourced from a climatologically ~~important~~significant eruption rather than a smaller Icelandic one. Most importantly, ~~M~~more climate modelling studies of high latitude, large, and ~~high~~ sulphur-rich eruptions under deglacial boundary conditions are ~~important~~needed ~~for~~ to constrain~~ing~~ the climate effects of volcanism during deglaciation~~their climate effects, and test this hypothesis~~. The role of halogen emissions are particularly understudied, and modelling efforts should also quantify their effects on climate.~~ ~~ Finally, accurate dating of other large volcanic eruptions during intermediate ice volume conditions is key to testing the link between volcanism and other Greenland stadials; this information could eventually also support, or refute, the LSE's role in triggering the YD.

More research is clearly necessary to better characterize the sensitivity of Last Glacial climate to volcanic eruption latitude, sulphur content, and magnitude. ~~Accurate dating of other large volcanic eruptions during intermediate ice volume conditions is key to testing the link between volcanism and Greenland stadials; this information could eventually also support, or refute, the LSE's role in triggering the YD. Despite these uncertainties, the currently available evidence strongly supports the~~ ~~Despite the fact that the original version of the hypothesis was one of the first proposed explanations for the YD, d~~Due to perceived chronological mismatches, ~~it~~the concept that the YD was triggered by the LSE- is vastly understudied compared to both the Younger Dryas Impact Hypothesis and the meltwater forcing hypothesis. However, the concept that the LSE triggered the YD has clear advantages compared to other hypotheses: *i)* there is no disagreement that the eruption occurred, *ii)* available evidence suggests that the eruption occurred synchronously with the initiation of YD cooling, *iii)* a volcanic trigger is consistent with the relatively high frequency of similar, and ~~occasionally~~often more severe, cooling events during intermediate ice volume conditions, *iv)* volcanic aerosol cooling followed by a prolonged positive feedback has been implicated in other cooling events, and *v)* events similar to the YD occurred over other deglaciations, seemingly arguing against an extremely low-probability, high-consequence event as a trigger, but supporting a relatively commonplace trigger~~,~~ such as volcanism. ~~(Buizert et al., 2014)~~Future research may well demonstrate that the Laacher See eruption did not play any role in catalysing the YD, but the coincidence of a large,~~n~~ high-latitude, and anomalously sulphur-rich ~~unusually sulphur-rich, Pinatubo-sized~~eruption ~~eruption~~ with the initiation of YD-related cooling certainly merits a closer ~~examination~~look~~a closer look~~. ~~concept that the Laacher See eruption played a key role in catalysing the Younger Dryas event.~~

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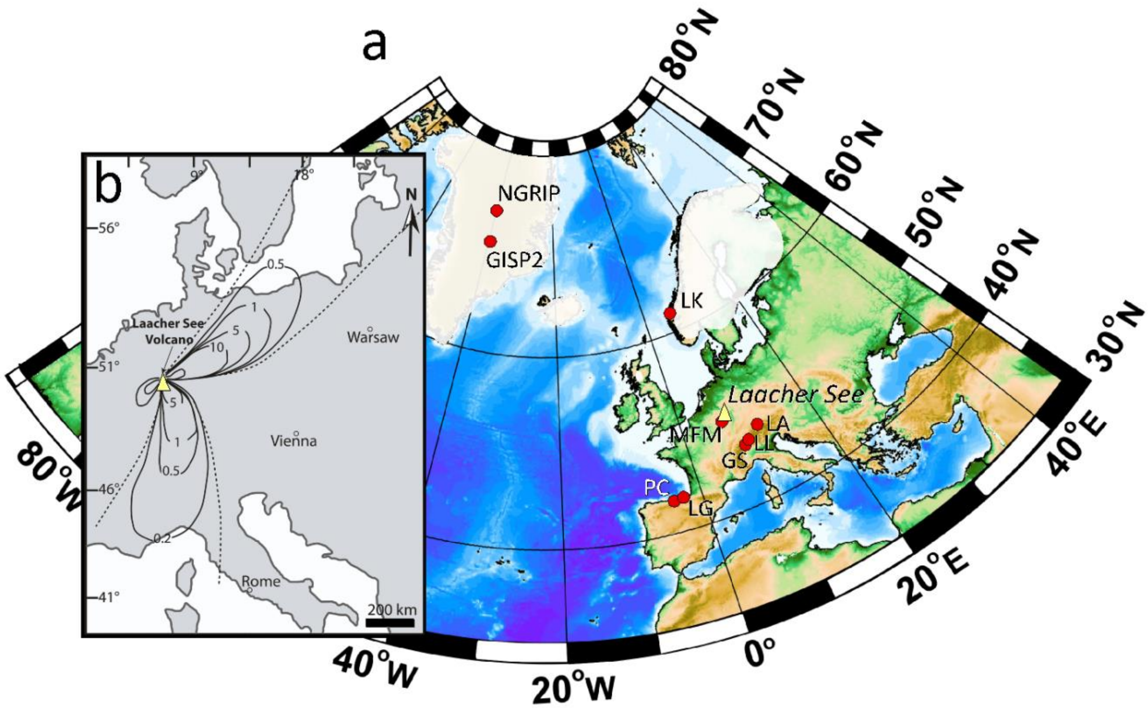

Figure 1. a) Map with the locations of sites discussed and (b) an isopach map of the Laacher See tephra fall deposits across central Europe. Semi-transparent white areas in (a) demarcate continental glaciers, and the reconstruction and map are adapted from Baldini et al. (2015b). The sites shown are: Laacher See volcano (yellow triangle); LG, La Garma Cave; MFM, Meerfelder Maar; LA, Lake Ammersee; PC, El Pindal Cave; LK, Lake Krakenes; LL, Lake Lucerne; GS, Gerzensee; NGRIP, NGRIP ice core. The dashed line in (b) is the outer detection limits of the distal tephra layers (adapted from Bogaard and Schmincke, 1985). Isopach line labels in (b) are in cm.

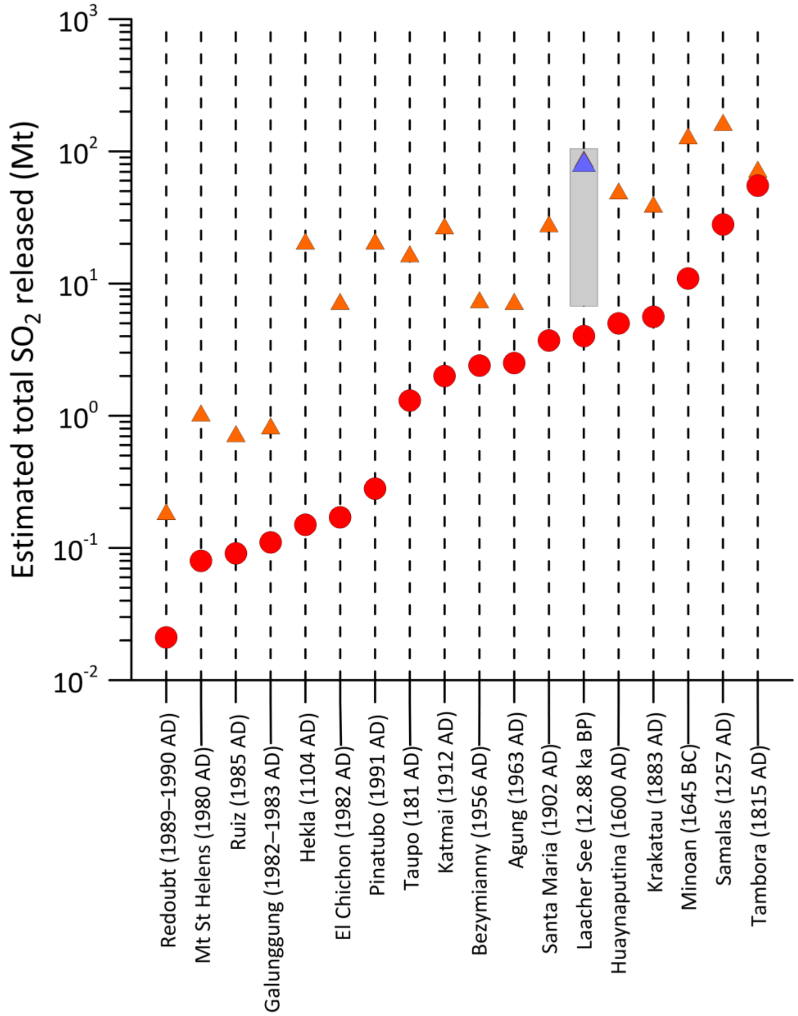

Figure 2XX6: Shifts in Nordic Sea sea ice extent and meltwater influx. Cabedo-Sanz and Muschiatello dataThe SO_2 yield of climatologically significant late Holocene eruptions (Shinohara, 2008). Red circles represent petrologic estimates of SO_2 release, and orange triangles represent estimated actual'observed' values, calculated from either satellite or ice core data (Shinohara, 2008). The grey bar represents the range of values for the Laacher See eruption as suggested by Textor et al. (2003), and the blue triangle in the Laacher See column represents the total estimated SO_2 emitted by the Laacher See eruption (83.6 Mt) assuming that the actual SO_2 emitted is 20.913.9 times the petrologic estimate, ; this value is calculated here as the mean value of 176 non-basaltic exposliveexplosive eruptions (the 16 listed in Shinoihara (2008) plus the 1257 AD Samalas eruption (Vidal et al., 2016)). The high standard deviation however of

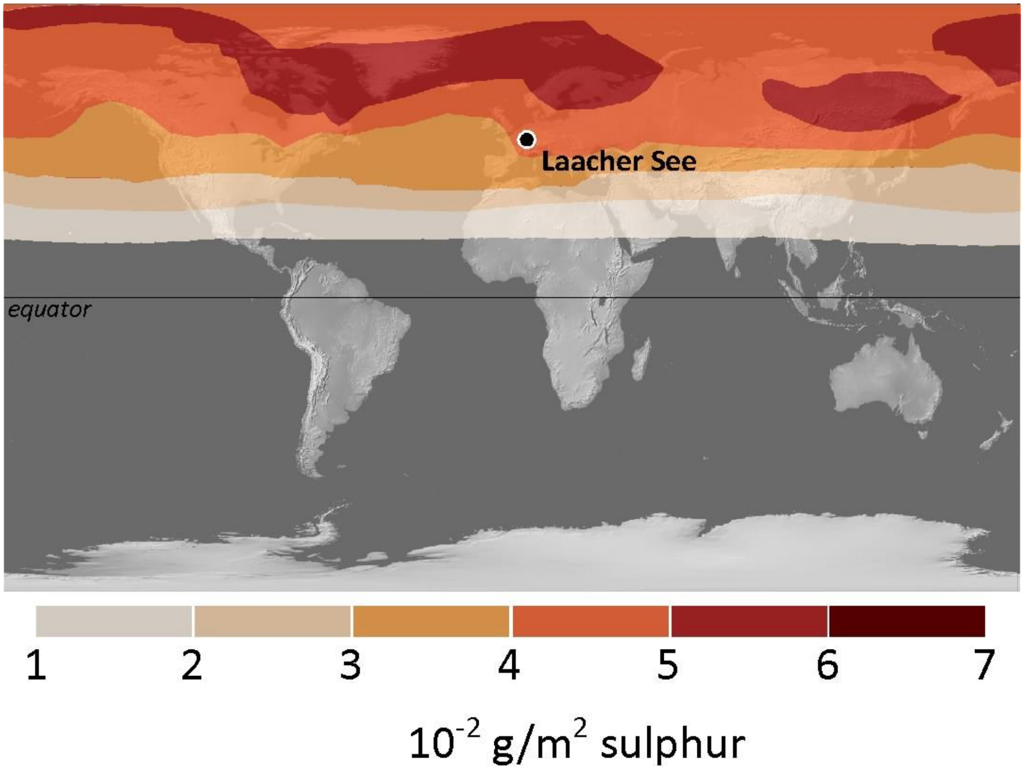

Figure ~~2~~3. The distribution of the Laacher See volcanic cloud six months after the ~12.9 ka BP eruption based on existing climate model outputs (MAECHAM4 model) (Graf and Timmreck, 2001). The figure is redrawn based on the original in Graf and Timmreck (2001), which contains details regarding the model and simulation. T~~Note that t~~his simulation assumed that the eruption injected 15 Mt SO_2 into the lower stratosphere, ~7 ~~15 megatons of SO_2~~x less than the maximum estimate of Textor et al. (2003a) ~~into the lower stratosphere, compared with more recent estimates of up to 150 megatons sulphur (Baales et al., 2002; Mortensen et al., 2005; Bereiter et al., 2018)~~.

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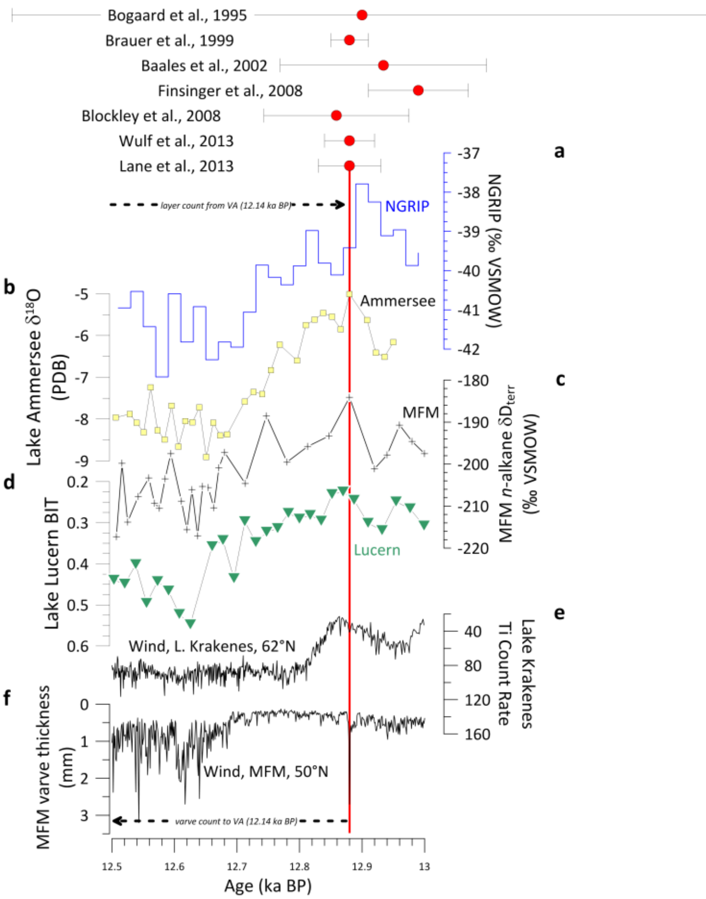

Figure 43. Temperature (a-d) and wind strength (e, f) proxy records from the North Atlantic and Europe. a) NGRIP ice core $\delta^{18}O$ (Greenland, GICC05) (Rasmussen et al., 2006). b) precipitation $\delta^{18}O$ reconstructed using deep lake ostracods from Lake Ammersee, southern Germany. c) Meerfelder Maar (MFM) (Eifel Volcanic Field, Germany) *n*-alkanes δD_{terr} (terrestrial lipid biomarkers) (Rach et al., 2014; Litt et al., 2001), d) Lake Lucern, Switzerland, isoprenoid tetraether (BIT) record (Blaga et al., 2013). e) wind strength as reconstructed using Ti count rate in Lake Kråkenes (Norway); three-point moving average shown. f) Wind strength as reconstructed using MFM varve thickness data (Brauer et al., 2008; Mortensen et al., 2005). The records are arranged so that cooling is down for all the records. The LST (vertical red line) is present or inferred within the MFM, Lake Ammersee, and Lake Lucern cores. The LST is not evident in the NGRIP or Lake Kråkenes cores, and the eruption's timing relative to NGRIP $\delta^{18}O$ and Lake Kråkenes Ti is based on layer counting from the Vedde Ash ('VA'), a tephrochronological marker (12.140 ± 0.04 ka BP) also found in MFM (Lane et al., 2013a; Brauer et al., 2008). The published chronologies for Lake Ammersee and Lake Lucern were shifted slightly by a uniform amount (0.115 and -0.093 ka, respectively) to ensure the contemporaneity of the LSE in all the records. This adjustment does not affect the LSE's timing relative to the Lake Ammersee or Lake Lucern climate records. Published radiometric dates for the LSE are shown (red circles) with errors, although the absolute age is not as important as its timing relative to the apparent climate shifts.

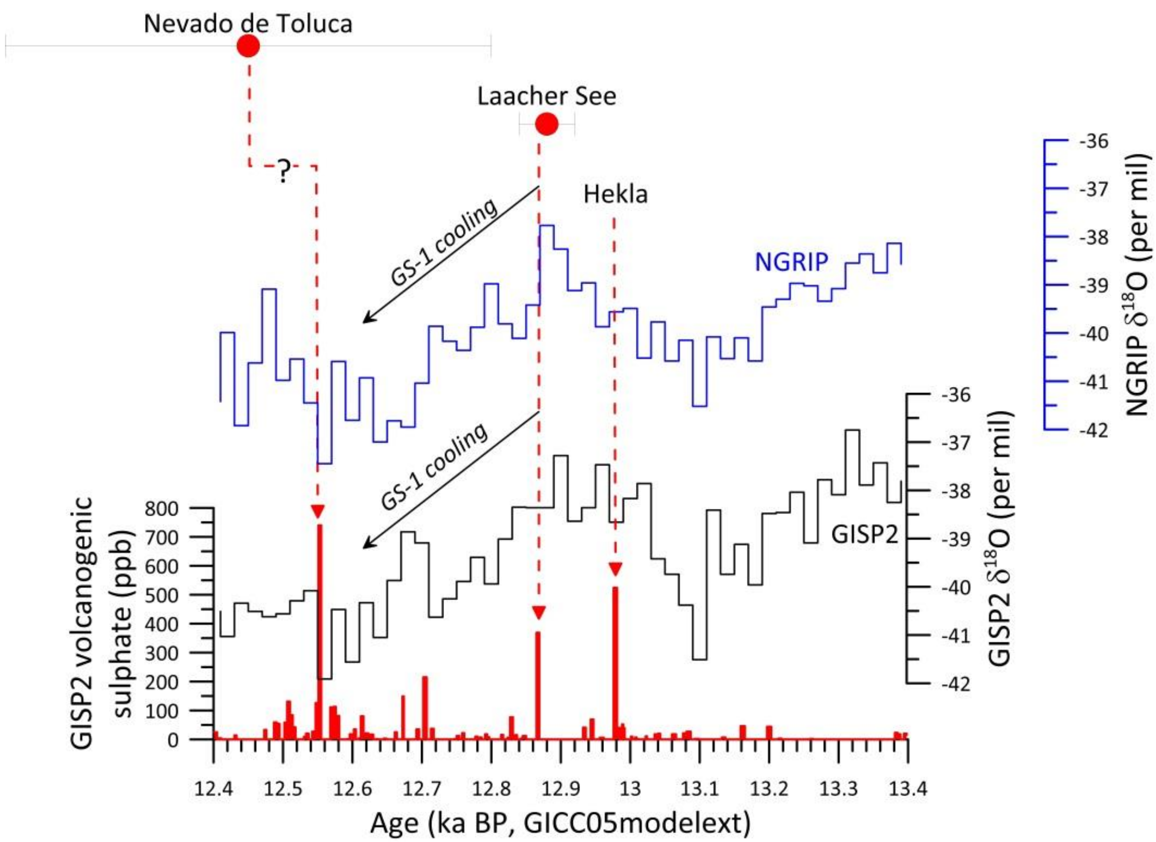

Figure 54. The GISP2 (bidecadal (Stuiver et al., 1995)) (black) and NGRIP (blue) ice core $\delta^{18}O$ records synchronised on the GICC05modelext chronology (Seierstad et al., 2014). The red bars indicate GISP2 volcanological sulphfate record (also synchronised on the GICC05 timescale). The maximum counting error of the ice core chronology is 0.138 ka at 12.9 ka BP (Seierstad et al., 2014). The best age estimate of the LSE is shown (red circle) (Brauer et al., 2008; Lane et al., 2015) and a possible correlation to a synchronous sulphfate spike highlighted by the vertical dashed arrow. The large spike at ~13 ka BP represents a smaller but more proximal Icelandic eruption (Hekla) associated with a volcanic ash layer (Mortensen et al., 2005; Bereiter et al., 2018).

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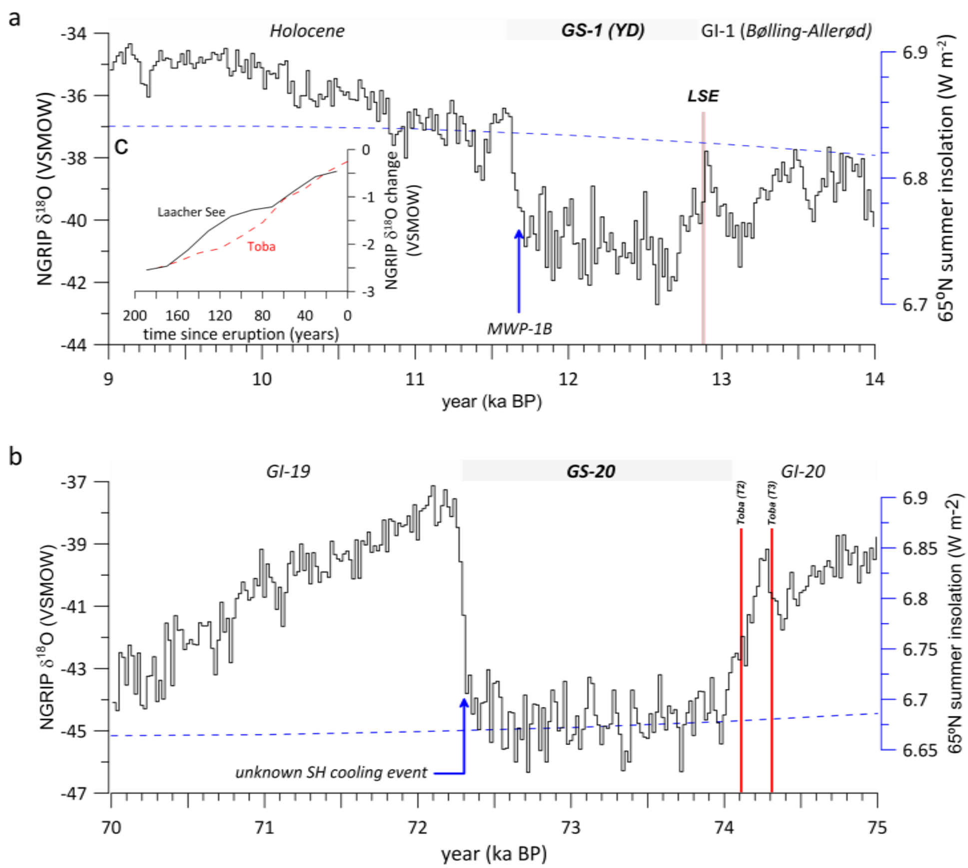

Figure XX~~6~~: Shifts in Nordic Sea sea ice extent and meltwater influx. Cabedo-Sanz and Muschiatello data. ~~Two five-ka intervals of the NGRIP $\delta^{18}O$ record bracketing (a) the ~12.9 ka BP Laacher See Eruption and (b) the ~74 ka BP Toba supereruption (Rasmussen et al., 2006). The two eruptions are among the most well-dated eruptions of the Quaternary. The LST is not apparent in the NGRIP core (although we identify a candidate sulphate spike in the GISP2 record; see Figure 4),~~

Svensson et al. (2013). Blue arrows indicate the timing of possible SH cooling events. The timing of Meltwater Pulse-1B is based on Ridgwell et al, 2012, but the source, ~~and~~ timing, and even occurrence of the meltwater pulse are still debated. The inset panel (c) shows NGRIP $\delta^{18}O$ during the 200 years immediately following the two eruptions (the sulphate spike T2 is assumed to represent Toba, as suggested by Svensson et al. (2013)).